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[SECOND SERIES.]

ART. I.—*Eulogy on Von Buch;* by Professor COTTA of Freiberg.*

AT Freiberg on the 19th of March was held a funeral festival in honor of LEOPOLD VON BUCH, the most celebrated geologist of our time, who died in Berlin on the 4th of the same month.

At 7 o'clock, P. M., the scientific audience collected in the Wernerian Hall of the Academy of Mines, which unfortunately did not possess extent enough to allow of the unusual attendance of the public at large. Professor Heuchler had decorated the cenotaph with the coat of arms and portrait of the deceased. Fitting geological specimens surrounded its base, which all had especial

* Professor SILLIMAN.—*Sir*—The accompanying translation of an eulogy pronounced in the Academy of Mines here, by Cotta, Professor of Geology, I have taken the liberty of sending you for your scientific Journal.

It is taken from the Leipzig "Illustrirti Zeitung" of April 9th, where the proceedings at length, with the illustrations, are given. The speech on the part of the students I have left out; and you may also omit if thought better, the introductory remarks of Prof. Breithaupt accompanying the translation. I have translated the address because it gives a true portrait of the remarkable man by one who knew him intimately, and also that his labors may be better known than they perhaps are in America, where he desired so much in his latter days to go, and of whose munificent patronage of science he ever spoke in unbounded praise. I had the honor of making his acquaintance last winter through the kindness of Prof. Cotta, while in Berlin, and of learning from him more of the general features of American geology with which he had of late particularly employed himself, than I had expected to do in Europe. He expressed but one regret, and that was that he could not be young again to go there and devote himself to the development of her geological resources. I have the honor to be, Sir, your ob't serv't,

J. GRAEME ELLERY.

Royal Saxon Acad. of Mines, Freiberg, June 4th, 1853.

reference to his scientific labors—such as ammonites and terebratulas, lava masses and other eruptive rocks, jurassic limestone, and chalk, druses of crystals and fossil leaves from the brown coal.

The mining and smelting officers of the crown, the professors of the Academy, in uniform, together with the students, at present uncommonly numerous even from the most distant foreign lands, filled the academic halls.

Professor BREITHAUPT first ascended the tribune and opened the exercises with the following remarks:—

Honored Friends! An aged, venerated and lordly oak in the German grove of science has fallen. Leopold von Buch is no more! Deeply do we feel this painful loss, the more deeply, as we may justly say, that in many respects, he belonged especially to us. Already in 1790 he was matriculated at the Academy of Mines at this place, and here his gifted genius laid the foundation of its subsequent comprehensive attainments. He ever remained true and attached to his alma mater, kindly communicating the rich fruits of his profound observations and investigations, and supporting without intermission a friendly intercourse with the literati of Freiberg. Only three years since, at Werner's festival held at this place, where he was the foremost ornament among those then present, he proved his old and honorable attachments by a noble act of munificence. To such a man are we in duty bound to pay a public token of our reverent acknowledgment and of our warmest thanks; and for that purpose are we here to-day convened—at this festival to his memory.

Through Professor Cotta we shall forthwith learn how much and what signal service the great geognost and geologist has rendered to science. The extent of Von Buch's merit will ever cause his name to shine forth bright and brighter as a star of the first magnitude, not alone in Germanic literature, but also whereever his favorite science may find a votary. By *us* can he never be forgotten—to us he will be ever peculiarly near and dear.

Then followed the eulogy by Professor COTTA.

We stand here before the manes and before the portrait of a man who devoted his whole life to Nature—of a man who once was Freiberg's and Werner's scholar, and whose fame now resounds far away over Germany's and Europe's boundaries, whose memory we will love, and so much the more ardently honor and celebrate, as he went forth from this school of mining life.

Leopold Baron von Buch was born on the 26th of April, 1774, probably at the old ancestral castle of the family at Stolpe, in the Uckermark in Prussia. Scarcely 16 years of age, he entered, on the 10th June, 1790, Freiberg's walls, where, under Werner's especial guardianship, and partially in his own house, he spent

three years. Here it was, that an intimate and long enduring friendship was formed between him, Alexander v. Humboldt, and Carl Freisleben. After the completion of his mining studies, he it was who most of all next to von Humboldt gave origin to that beautiful saying of d'Aubuisson de Voisin, that "Werner's disciples scattered themselves over every land and interrogated Nature as to her import from pole to pole." He also it was, however, who first of all brought back a negative answer from these wanderings. He went forth into the world a true and convinced disciple; soon, however, fact after fact accumulated before his clear vision, till he was convinced, at first, doubtless, painfully convinced, that his much loved master must have erred in one fundamental point.

We all are subject to error; and those who come after us will certainly know more, and know it much better, than we. It can therefore on that account never be made a subject of reproach to a disciple, that he has given up the system of his master from conviction; and it were also the worst way of honoring great men, to say nothing of its opposition to the spirit of true science, were we blindly to cling to all of their errors. On the contrary the acknowledgment of an error, or the discovery of a new truth on the part of the disciple, is ever a proof of the ability of both. The master has awakened a spirit of independent investigation—the disciple, by that, has laid down a proof of his own impartiality and of his independence. And never can a disciple in such a case act with more delicacy and forbearance against a master than did v. Buch, who never meanly attacked the *old*, but on the contrary presented the *new* only with more convincing power.

L. v. Buch wandered over,—and that too mostly on foot, as it behooves a naturalist to do,—one after another, not only all the mountain chains of Germany, the Alps from Nice to Vienna, the Appenines from Turin to their most southern spurs, the hilly chains of England and the highlands of Scotland, but he was almost as much at home in France as in Germany. He traversed again and again the Pyrenees, climbed the summit of Ætna, and the sublime Peak of Teneriffe. He had early wandered among the crystalline mountains of Scandinavia, and late in life even, when almost an old man, he wandered all over the Highlands of Greece, till then better known to the philologist than to the geologist. Everywhere, even where he only transiently tarried, he left behind him, as v. Humboldt says, luminous and radiant footsteps.

Thus was he ever, year after year, up to his advanced old age —to the very end of his days, on journeys. In early summer he wandered forth, and only with the storms of autumn did he return to his neat ground-floor study on the banks of the Spree,

where a cheerful domestic establishment surrounded the worker, with nothing however of that luxury which might have been expected from a man of his rank and opulence. The faithful Mrs. Baumgarten—known to almost every geologist—had been for more than twenty years his only attendant; and it very often happened that he himself opened the door to the one who rang. Were it a stranger who then had come, and asked, "Is Mr. v. Buch at home?"—he received an answer according to circumstances—"I will see"—or, "what do you wish of him"—or, "No." Even a well known acquaintance he sometimes also asked, "To whom do you come?"—or, began immediately even in the portal the conversation—generally with a question—on some determinate scientific subject. Once, while I was engaged in the geological survey of Thuringia, and visited him, he asked, even in the house-door, "Now does Ammonites semipartitus occur also in Eichsfeld?"

I have purposely made mention of these apparently insignificant circumstances, because they characterize the man, and because the last especially, shows the great activity of his mind, which, in moments of the greatest surprise, forthwith and omitting every introductory phrase, began at once at the very heart of the subject—at all times interesting to the new comer. However, I shall soon have occasion to speak of many peculiarities which not a little characterized the departed.

Permit me next to cast a glance at what science has gained from his life and genius. It would engage our attention for many an hour were I to enumerate all for which geology is indebted to him, who also in the departments of natural science as elsewhere, has given manifold proofs of a genius deep searching into the very heart of things. For it is especially characteristic of his labors, that he never lingered over trivialities, but knew how to discern at a glance the essential from the unessential, to render the characteristic distinctly characteristic, and to seek out the true connection of phenomena. It is less difficult even for an acknowledged obtuser mind, to make numerous and accurate observations, than through these to discern the legitimate in nature. This latter gift, however, was lent to L. v. Buch in a degree possessed only by the few.

The copiousness of his discoveries, however, will make it necessary for me to limit myself, at this time, to those alone, which appear the most important.

He it was who first of all in Germany proved with precision that the disturbance of the original relations in the deposition of strata cannot be explained alone by events on the surface, but that these have had in most cases a deep seated subterranean, plutonic or volcanic origin. He showed by little and little that not only the basalt, but also the other crystalline massive rocks have been

pressed upward from below in a molten condition like lava; and that by these same reactions of the earth's interior, the elevation of mountain chains, and of whole districts of country have been brought about.

He first directed attention to the determinate, and often reciprocally parallel, direction of lineal mountain chains, or extensive dislocations of strata. He distinguished four principal lines of elevation in Germany, upon which generalization Elie de Beaumont afterwards building, founded his artificial system of mountain lines, which, however, in its last form was not admitted even by L. v. Buch himself. He was the first of all to show that the large volcanoes had had their origin not alone from the simple heaping upon one another of lava streams and loose ejected masses, but that they had been elevated to a higher altitude together with the consolidated masses before present. In this manner he distinguished craters of elevation, and craters of eruption; and if in this last division he may in isolated cases have gone too far, still the most essential part of his doctrine will always remain of the highest importance.

Having once had his attention directed to the effects of volcanic activity, he investigated the mode of distribution of volcanoes upon the earth, registered upon charts all known ones, and showed that they were distributed partially in groups and partially in lines of which the last mentioned evidently were arranged upon long extended lineal cracks in the earth's crust. These investigations were first communicated in his splendid work on the Canary Islands.

Besides the local effects of the present volcanic activity as it is there developed where active volcanoes exist on the earth's surface, he early recognized also the effects of this same force in its more universal and less distinctly remarked phases. He it was who first of all in Germany proved that the continuous remarkable changes of level on many of the Baltic coasts cannot have their origin, as was generally believed to be the case, in the sinking of the sea, but that they are to be explained only by a gradual secular elevation of a great part of Sweden. And this view has since then been established beyond a doubt by an earlier opposer of it, Sir Charles Lyell.

I will not linger long over his theory on the part which the Melaphyre has played, and its influence on the formation of Dolomite, because this of all of his new views is perhaps most subjected to doubt. But even if this whole hypothesis should fall to the ground, still it was at any rate put forth with so much spirit and ingenuity, that it earned and obtained at the time, the highest attention, and in the most lively manner drew the attention of others to new investigations. In general the beautiful, lively and convincing manner of representation in all v. Buch's

labors is not the least part of their merit. In so simple and clear a style can no one write who is not perfectly master of his subject. Somewhat characteristic of him is it also, that he always shunned all references in the text, and with right, for they never belong to the embellishments of a book. They are generally only a consequence of the fact that the author is not able to spin out into one thread all that he would say, or that he has hung them on to prove his erudition, or they are entirely superfluous and do not belong to the subject. As they are impossible in the flowing speech, they should also be avoided as far as possible in written productions.

During the period of his long scientific career, occurred the discovery of the true meaning and the geological worth of organic remains; which till then had been looked upon as unessential things and had been but little noticed. Scarcely was their true import known than L. v. Buch entered upon this new department with the greatest zeal and happiest result.

First of all he devoted himself to the Ammonites, long however, before that peculiar delineations had been observed upon the surface of their interior petrified casts. Their true import and legitimate cause however had till then remained a mystery. He showed that they came from the walls of the interior concameration and at the same time pointed out the peculiar laws of their development, which for primeval zoology and for geology has become alike equally important.

He next turned his attention—ever seizing first of all upon what was at the time most important—to the genera Terebratula, Spirifer, and Productus, which, as paleontological remains, are found so extremely abundant in all formations, but which of all this division of the Brachiopods, formerly so numerous in species, are represented in the present seas only by a few species of the first named genus. Also here he discovered the determinate laws of form which stand in the most intimate relations to the mode of life of those remarkable animals. In a similar manner he later elucidated the Cystidea—a remarkable division of the Radiata—while earlier he had already described, in a splendid work, the fossils collected in America by Alexander von Humboldt and Ch. Degenhard, in these cases animating by his genius those long extinct forms of a primeval world as if they were still sporting amid the living.

The study of Organic Remains, which has given to geology an entirely new direction, also led him, who first introduced the conception of characteristic fossils for formations, to the more precise study of the strata in which they occur. In an extensive work he showed the nature and extent of the Jura formation in Germany. In this he proved that its strata had been deposited around a primeval continent, and by this pointed out the former division

of land and water in this part of the world at that time. He also subsequently showed, that the depositions at the period of the chalk—at least its organisms—are limited to a determinate zone of the earth's surface, extending, neither in the old world nor the new, beyond 60° of north or south latitude, which, if confirmed, will be the oldest proof of a division of zones upon our planet.

Last of all, the deposition and distribution of the brown coal formation in Germany as well also as that of the chalk of North America engaged his attention. With the investigation of the former were connected peculiar studies of a botanic nature which had also occupied him for many years, but which he had now so far perfected as to bring their result to bear upon the subject in question. The fossil impression of dicotyledonous leaves, whose exact determination is often extremely difficult, led him to the study of the living forms of leaves. He rested seldom under the shadow of a tree without accurately observing the structure of its leaves and numbering their nerves. He collected hundreds in a small herbarium and by continued comparison succeeded in discovering a determinate law in the arrangement of their nerves, according to which all leaves arrange themselves under the four divisions of "Randläufer, Bogenläufer, Spitzläufer, and Saumläufer."

Here permit me to leave the succession of weightier and in part more brilliant discoveries, for which the physical sciences, and especially geology, whose reformer he was, are indebted to him, to return once more to the personal, where I may not and cannot avoid a more subjective representation, inasmuch as I have had the fortune of not being a stranger to him, and in many ways know his high manly worth.

You all may have heard doubtless of many a peculiarity of L. v. Buch, who at times under a stern exterior, always, however, bore a deeply sensitive and noble heart. Great men are seldom without sharply defined and deeply stamped peculiarities, and these then belong obviously to the full completion of the portrait.

The custom of performing all his journeys as far as possible on foot, without guide, without knapsack, in black dress coat, and round hat, in shoes and (formerly silk) stockings, all of which articles of dress being often from the hardships of the journey far more jaded than their bearer, brought him many a time in peculiar conflict with travellers, police authorities and landlords, from which, of course, by intellectual superiority and a good passport, he ever came off at last victorious. Hundreds of original anecdotes which have happened to him on his journeys are known. He himself appeared not unwilling even to relate them, and much as he was accustomed also to be importuned by such misunderstandings, still one can scarcely believe that he always shunned them.

He never communicated in advance, when and where he should travel; and even as he began his extensive journey to the Canary Islands, no one in Berlin had the least intimation of it before his departure. The mechanician who had to construct his barometer, could only conclude that he meant to ascend to high altitudes, as it must be arranged for the determination of heights of 14,000 feet.

I believe myself not to be inexperienced in wandering on foot, but I must however acknowledge, that I was right glad late one evening to have reached the terminus as we once sixteen years ago, wandered thirty-five miles over the mountains from Schandau to Tharand. We halted for refreshment but once on the way, at a spring whose waters we quaffed from the goblet of Diogenes. Instead of stopping at an inn to rest, he halted with pleasure but once in a beautiful spot in the freedom of nature, but even there not without investigating a stone or a leaf in the meanwhile. At such a time he once said, "If we contemplate with attention any one subject of Nature, we can always find something new in it, should it have been investigated and described ever so often."

In this way he generally travelled, seeking however of course at evening as good an inn as possible, with whose signs and characters in the greatest part of Europe there was scarely any one so well acquainted as he.

If, when in a distant land, winter surprised him, then indeed the way home on foot was no more practicable. To travel with strangers however, in a stage-coach, was to him, on account of the possibility of coming in contact with a smoker, fundamentally out of the question. He therefore, before railroads were known, used in every case to purchase his own waggon and with extra post horses return home. But now as he did not possess the gift of selling these again in Berlin, whole collections of all sorts of travelling vehicles here collected on his hands, until at length some relative resolved upon selling them for him. But enough of these peculiarities, which easily could be communicated in much greater numbers. They often form, however, only the original exterior of one of the noblest hearts.

Unmarried as he always was, and notwithstanding his being ever on journeys, L. v. Buch made use for himself of not the half of his large income. Believe not however in the least, that he hoarded up or collected the other half! He collected only the medals of creation, none stamped by the hand of man. He supported what appeared to him worthy of support, with a lavish hand, and that too without having it easily remarked. Not alone in the cause of science, but also in the purest philanthropy, he expended, doubtless, thousands yearly. I myself have seen him moved to tears at the misfortune of another, and I know the satisfaction of

having witnessed it without being able to prize it highly enough. He was accustomed to say at such a time, "He must be helped," and he was helped, by an unseen hand helped.

Perhaps it may be said by one or another, that it were quite easy for a man so independently circumstanced as he, to devote himself exclusively to science, and with so many means to have accomplished great results. With such, however, I cannot agree. Birth and wealth had opened to L. v. Buch many an easy way that would have led to a pleasant, yes, even a brilliant life. Hundreds, I fear, who, struggling with necessity, have earned for themselves a name in science, would, in L. v. Buch's circumstances, have chosen a path in life leading more directly and easily to commanding influence.

The spur of necessity is with many not a small one. But for a man of fortune without such extreme urging, to devote his whole life voluntarily to earnest, pure investigation, and only for that end, there is something in it, as it appears to me, of the great, something of the uncommon; for it is one thing to cultivate a branch of science incidentally, for pastime or amusement, or to acquire a certain credit for erudition, and quite another to resign oneself entirely and undividedly to it.

With such zeal for scientific advancement he also knew well how to draw forth youthful talent wherever he found it, to captivate it, lead it into a fitting path, support it by counsel or assistance, and with such delicacy of feeling and manner, that it itself scarcely perceived how much it was indebted to its patron.

He never accepted a public office, but bore occasionally on festive occasions the key of a "Kammerherr" and many a high order of merit.

I may also not pass by without mentioning the uncommonly varied character of von Buch's attainments, and the retentiveness of his memory even for trifles; it was his custom to note in his small day-book, often embracing the wanderings of many years, only brief remarks in a microscopic hand. Perfectly at home in five or six languages, he was also deeply read in history and literature. Even trivial family circumstances and town occurrences his memory retained in all their details, and he knew how to rehearse them in the most felicitous manner. His conversation was on that account not less spirited than fascinating, and he could, when he was in the right humor, enliven even the gayest saloon in the highest degree.

Now, however, one word as to his death. On Saturday the 26th February, he was till late at the Humanitätsgesellschaft—a conversational meeting of literati of Berlin. Professors Poggendorf and Braun accompanied him thence to his dwelling. At the door he bade them adieu with some jokes as usual. Upon retiring to rest he felt himself slightly indisposed. The next day began

the malady with violent pains in the foot, in which for many years he had suffered from chilblains. He did not afterwards leave his couch, and a letter from Dresden remained unread. On Tuesday the physician was called. The pains had left him, but a general debility, a nervous excited condition, had taken their place. His acquaintances, however, still knew nothing of his sickness. On Wednesday, Prof. Beyrich accidentally visiting him, received for the first time information of the sufferings of the highly honored man. He found him in bed, but cheerful, and joking in his wonted manner, alluding often to the task he had undertaken on the chalk formation of North America, and which had engaged his attention for some time past. Upon his writing table lay the beginning of his work with the superscription, "Nebraska," but under this however, were only two lines, probably written on Saturday. On the same evening, Prof. Beyrich carried the intelligence of his illness to the meeting of the German Geological Society, of which von Buch was president. During the night of Thursday his condition became much worse. Debility and fever had visibly increased. However, on Thursday he could still converse with most of those who visited him during the day. When Prof. Beyrich visited him again at 10 o'clock on Friday, March 4th, he found Messrs. Ewald, Braun, and Papiz already at the couch where he lay unconscious, and they did not again leave it till his death, which occurred at twenty minutes before two o'clock.

On March 9th the funeral solemnities took place in the dwelling of the departed, which the Royal Botanic Garden had richly decorated with palms and laurel. His mortal remains were then transported to the family vault at Stolpe.

You thus have a few fleeting sketches of the life and death of the man to whose memory we are here convened to pay our homage, and of whom we are proud, not only as Germans, but also as citizens of Freiberg. Yes, we are something more than proud of him, we are much his debtors. He has never forgotten his Freiberg. Each of his works—of which all never came into the hands of the booksellers—is to be found from his own hand in the Library of the Academy, and besides this also in many of the private libraries of the place. Before the Kreuzthore stands as an ornament of the city, Werner's bust, (a present from Count Einsiedel,) but for whose erection and embellishments we are indebted to von Buch's liberality. He often came here, and only three years since when we celebrated the memory of his distinguished master, he was to our joy with us in this hall, where to-day his portrait presents to us only the noble lineaments which we shall never forget, as his whole service must forever remain unforgotten.

An account of the origin of this portrait may not be without interest. L. v. Buch had often received urgent requests to allow

his portrait to be taken, but had never consented. When his friend Friesleben on the remittance of his own portrait, once urgently requested his in return, he received in its stead a large lithograph of an Ammonite with the signature under it, "Leopold v. Buch." There was little hope then, of obtaining a portrait of the great geologist, aside from the imperfect failure in the Dictionnaire des Sciences Naturelles. Some years since, however, his King sent the celebrated portrait painter, C. Begus, to him, and told him, that he, the King wished his portrait. What remained for him to do? He must obey and sit still. From that portrait is this lithograph a copy.

And now permit me to address myself to you who are dedicacating yourself to the same studies which here once lured this distinguished man. If you all however have not proposed to yourselves the same course—if you are not all called to furnish similar results in your particular departments like a L. v. Buch, still may you ever take him as an exemplar. His example like that of Alexander v. Humboldt, and many others, teaches at the same time that even from an unpretending place of study great effects may go forth. May we all strive to imitate him in untiring zeal, system, and noble sentiments—this will be the highest honor we can show to his memory. For the immortality of his name, he himself has provided.

At the close of the exercises, the band of the cavalry regiment, in garrison here, played a Dead March.

Art. II.—*Extracts from the Report on the Geology of the Lake Superior Land District;* (Part II.) by J. W. Foster, and J. D. Whitney.*

In Part I, of this Report, communicated to the Commissioner of the General Land Office in 1850, and published in 1851, we have given a historical sketch of the exploration of the country bordering on Lake Superior, a description of its physical geography and climate, and so much of its geology as was necessary to the full elucidation of the copper-bearing rocks and their relation to the sedimentary formations; this being the subject to which that part of the report was principally devoted. The two concluding chapters contained an account of the drift phenomena so conspicuously displayed in the region of the great lakes.

In Part II, of this Report, we shall proceed to the detailed and systematic description, so far as our materials will enable us, of

* The appearance of this valuable Report, was announced in volume xv, page 295. We here cite some paragraphs from the chapters, on the general characters of the rocks, and on the Azoic and the lower Silurian systems.

the geology of the whole of the Lake Superior Land District, commencing with those formations which are the lowest in the scale of geological succession, or those which were first formed, and ascending to those which are now in the progress of accumulation. We shall only allude to the results of the former part of the report, so far as it may be necessary to enable the reader to form a connected idea of the geology of the whole region.

The following table exhibits the names and the order of succession of the geological groups which have been recognized as existing within the limits of our district.

Classification of the Rocks.

FORMATIONS.	IGNEOUS.	*Of Various Ages.*	Plutonic Rocks.	Granite.
				Syenite.
				Feldspar and Quartz rock.
			Trappean, or Volcanic Rocks.	Greenstone, or Dolerite, Porphyry.
				Basalt, Amygdaloid.
				Hornblende and Serpentine Rocks.
				Masses of Specular and Magnetic Oxyd of Iron.
	METAMORPHIC.	*Azoic System.*		Gneiss, Mica and Hornblende Slate.
				Chlorite, Talcose and Argillaceous Slate.
				Beds of Quartz and Saccharoidal Marble.
	AQUEOUS.	*Silurian System.*	Lower.	Potsdam Sandstone.
				Calciferous Sandstone.
				Chazy Limestone.
				Birds-eye Limestone.
				Black-River Limestone.
				Trenton Limestone.
				Galena Limestone.
				Hudson-River Group.
			Upper.	Clinton Group.
				Niagara Group.
				Onondaga Salt Group.
		Devonian System.		Upper Helderberg Series.
		Drift System.		Beds of Sand, Clay and Gravel rudely stratified.
				Transported Blocks of Granite, Greenstone, &c.
		Alluvial Deposits.		Sand and Pebble Beaches, Marshes, Flats, Hooks, Spits, Dunes, &c.

The New York geologists have divided the Silurian system, as developed in that state, into eleven groups, while some of the Western geologists recognize, in its western extension, but five. Between the two systems of classification there is no community of names.

The geographical position of our district is such as to form a connecting link between the east and the west. While, on the

one hand, the New York and Canadian geologists have traced the Silurian groups up to the eastern borders of our district; on the other hand, the geologists of Michigan, Iowa, Wisconsin and other states, have traced the same groups, though under different names, along their southern and western prolongation, without having attempted to identify them with their eastern equivalents, or to subdivide them according to the palæontological evidence.

Under these circumstances, we have endeavored to connect the two sets of observations and blend them into one harmonious whole. As the New York survey is the only instance in which the matured results have been communicated to the public, and as the volumes on the palæontology—a monument of the research and perseverance of the author, Mr. Hall—will form the standard of reference for the whole country, in determining the succession of the Silurian groups, we have deemed it advisable to adopt that nomenclature, so far as the same groups described by the New York geologists could be recognized in our district.

The designation of groups of strata by names derived from their geographical position, or from the locality in which the rocks are first investigated and their relative position clearly defined, seems to be of all the methods of nomenclature that which, for the present at least, is liable to the least objection. Names given solely with reference to lithological character, or to the presumed predominance at any particular point of a certain genus or class of organic remains, seem much more likely to lead to misunderstanding and confusion; and, however desirable it may be that a universal system of nomenclature and arrangement should be introduced, it seems quite impossible to hope for any such thing in the present state of geological science, a science which is so rapidly developing, and liable to such constant changes. The names introduced by the New York geologists, are in most instances derived from the locality where the group designated is particularly well developed, and the fact that those groups have, in their continuation through Canada, been described by Mr. Logan, the Provincial geologist, under the names recognized by the New York survey, seems an additional reason for their adoption, as far as possible, by us.

It will be seen from the details incorporated in a subsequent part of this report, that many members of the Silurian series, particularly the grits and conglomerates, which are clearly defined in New York, have but a limited range, and disappear altogether before reaching the limits of our district. These are conditions which we ought to expect would exist in deposits made along a shelving ocean-shore; but so far as these are persistent, it seems desirable that they should bear the same names throughout their whole extent.

The following is the synonymy of the groups of the systems developed in this region, according to the nomenclatures adopted in the reports of the different surveys.

New York and Lake Superior.	*Pennsylvania and Virginia.*	*Ohio, Iowa and Wisconsin.*
AZOIC SYSTEM.		
AZOIC SYSTEM. (Not classified in New York.)	AZOIC SCHISTOSE SERIES.	METAMORPHIC ROCKS. (Wanting in Ohio and Iowa.)
SILURIAN SYSTEM.		
Potsdam Sandstone.	Primeval Sandstone, or Formation No. 1.	Lower Sandstone, or Formation 1, wanting in Ohio.
Calciferous Sandstone.	Lower part of the Matinal Series, or part of No. II.	Lower Magnesian Limestone, Formation 2, wanting in Ohio.
Trenton Group (including Chazy, Birds-eye, and Black-river Limestone.)	Middle part of Matinal Series, or part of No. II.	Fossiliferous Limestone, No. 3. Blue Limestone and Marls of the West.
Galena Limestone (not recognized in New York.)	Not recognized in Pennsylvania and Virginia.	Erroneously regarded as the equivalent of the Cliff or upper Magnesian limestone.
Hudson-river Group.	Matinal Shales, or No. III.	Associated with No. 3, or the Blue Limestone and Marls of Ohio.
Medina Sandstone and Clinton Group.	Part of the Levant Series, or part of No. V.	Not recognized at the West.
Niagara Group.	Part of the Levant Series, or part of No. V.	Cliff limestone of Ohio and Indiana. Upper Magnesian Limestone.
Onondaga Salt Group.	Summit of the Levant Series.	
DEVONIAN SYSTEM.		
Upper Helderberg Limestone.		Upper portion of the Cliff Limestone.

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Azoic Series on the Northern Shore.—The rocks of which it is composed are developed on an extensive scale, both on the northern and southern margin of Lake Superior basin. Commencing on the northern shore of the lake, we find a series of talcose and chlorite slates with occasional beds of coarser grits, in immediate contact with the granite and gneiss. They have been divided by Mr. Logan, the distinguished Provincial Geologist of Canada, into two groups—a division which we have failed to recognize on the southern shore—the lowest of which consists of slates partially chloritic and talcose, and occasionally holding a sufficient number of pebbles derived from the hypogene rocks to constitute conglomerates. "These slates," he remarks, "are of a dark-green color, often dark-grey in fresh fractures, which at the base, appear to be occasionally interstratified with beds of a feldspathic quality, of the reddish color belonging to the subjacent granite and gneiss: sometimes they are a combination of feldspar and quartz, occasion-

ally with the addition of hornblende, making syenitic beds, and in some the hornblende predominating gives the syenite a general green tinge. Some of the beds have the quality of a greenstone, others that of mica slate, and a few present the character of a quartz rock."* These slates, he conjectures, attain a thickness of several thousand feet, and are well exposed at the mouth of the river Doré, about five miles from the Michipicoten river. The strike of the beds is very irregular and their dip highly inclined.

The upper group rests *unconformably* on the preceding, and towards the base presents conglomerate beds of no great thickness, the pebbles of which consist of white quartz, red jasper, and occasionally slate, the whole enclosed in an arenaceous matrix. Higher up are found layers of chert, occasionally approaching chalcedony. The plates are separated by thin calcareous seams, presenting a ribbon-like appearance.

In the vicinity of the disturbed parts, the chert sometimes passes into chalcedony and agate, and small cracks are filled with anthracite, which is also found forming the centre of minute globules, enclosed in a silicious matrix.

Higher up in the series, the argillaceous slates become interstratified with argillaceous sandstones, in such an altered condition that it is often difficult at first sight, to say whether the latter may not be trap layers. Calcareous bands occasionally occur of sufficient purity to be called limestone. Interlaminated masses of trap are found near the base and overlying the summits. They are composed of a black hornblende and greenish-white, horny-looking feldspar, in no instance assuming an amygdaloidal character, but occasionally presenting a porphyritic appearance. It exhibits a sub-columnar structure, and the crowning overflow of trap communicates a peculiar aspect to the whole region occupied by this formation. It attains a thickness of nearly 2000 feet, and, where it comes to the lake, rises in bold, overhanging cliffs.†

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Azoic Series on the Southern Shore.—In the region included between the two great lakes, known as the northern peninsula of Michigan, this group constitutes the fundamental rock. The materials of which it is composed appear to have been thrown down in a comminuted state, since we rarely meet with those grits or conglomerates which occur on the north shore. They constitute alternating beds of great thickness, known as gneiss, hornblende, chlorite, argillaceous, silicious and talcose slates, quartz, saccharoidal, and crystalline limestones. They are highly inclined and much contorted, and nowhere exhibit the characters of a purely sedimentary rock; but the evidences of metamorphism are more striking as we approach the lines of igneous outburst.

* Report of Progress, 1846–7, p. 10. † Ibid. pp. 13–14.

Gneiss generally flanks the granite, succeeded by dark masses of hornblende, with numerous joints, but obscure lines of bedding, which often graduates into hornblende slate or chlorite slate, as we recede from the purely igneous products.

The outlines of this class of rocks are extremely irregular, and a reference to the general map will give a clearer idea of their range and extent than a mere verbal description. * * *

In this district, the area occupied by these rocks exceeds eighty townships, or more than three thousand square miles. The configuration of the slates and granites may be compared to the contours of a rugged coast. The main granite masses form numerous projecting headlands, while the subordinate patches rise up like islands. The slates sweep round the promontories and form numerous narrow and deeply-indented bays.

The topographical features of the region occupied by the slates are striking. It is diversified by bold, rocky cliffs and narrow intricate valleys, with lakes and water-falls, with luxuriant forests and natural meadows. The culminating points reach nearly twelve hundred feet above Lake Superior or eighteen hundred feet above the ocean-level.

We commence our description of the local phenomena of this system of rocks where they intersect the lake shore between Riviere du Mort* and Chocolate river. The sketch entitled, "View near Carp river" forming the frontispiece [see Report], may serve to convey an idea of the contours of this region better than a written description. The lake here forms a spacious bay with gently curving shores. A range of quartzose hills rising to the height of six hundred feet, terminates abruptly by the coast and forms the background of the picture. The extremity of the point consists of sand-dunes rising to the height of fifty or sixty feet, with rounded outlines and highly inclined slopes towards the lake. Along its margin are to be seen the remains of ancient terraces which indicate its former limits. The middle ground is occupied by a range of trappean rocks interlaminated with the slates. The settlement represented, has been named Marquette, in honor of the early missionary, and has already become the main outlet of the Iron region. The foreground is composed of another rocky promontory which projects for some distance into the lake, and serves as a shelter to vessels against a west and northwest wind; while by the shore, vestiges of the Silurian sandstone are seen reposing upon the upturned edges of the azoic rocks. It would be difficult to select another spot, along the whole coast, where the

* This river is generally called *Dead* river. It cannot be from the sluggishness of its current, for in the distance of thirty miles, it falls more than a thousand feet, abounding in rapids and cascades. Its true name is the River of Death. There is a local tradition as to some act of violence here perpetrated, which we cannot now recall, and from which the river derives its name.

rocks of so many epochs, from the oldest to the most recent, are represented. It contains an epitome of nearly the whole geology of the district.

The quartz zone exhibits two distinct ridges, where it approaches the lake, hemming in the valley of Carp river with rocky walls, from two to six hundred feet in height. As we trace it westwardly, it presents but a single ridge, and after having passed Teal lake, sinks down and becomes lost. Where exposed by the lake shore, it exhibits lines of bedding and obscure traces of ripple marks. These lines bear east and west, and dip 86° to the south, while the Potsdam sandstone abuts against the quartz, in a nearly horizontal position. Some of the quartz beds in this vicinity enclose fragments of jasper and slate, showing that they contain vestiges of prior-formed rocks. The southern ridge—using the notes of Mr. Hill—presents a number of conical knobs rising from two to three hundred feet above the surrounding country. In section 2, township 47, range 25, a granite boss rises above the quartz, over which the strata are folded like a mantle. In the northeast quarter of this section, a band of slaty limestone, somewhat silicious, is seen beneath the quartz, bearing northeast and southwest, with an inclination of 44° to the southeast. In the northern part of section 3, the quartz is observed, with another band of limestone interstratified, bearing nearly west-northwest. The protrusion of the granite has displaced the beds and broken their continuity; one portion shifted to the south, was traced as far as the line between sections 9 and 10, while another portion shifted to the north, was traced westward into section 4. The northern beds were found to be associated with a layer of limestone, or compact marble, only a few feet in thickness. The northern ridge attains a higher elevation than the southern, the highest point in section 6, being five hundred and ninety-two feet above the lake. The quartz has been so far metamorphosed as to destroy the lines of bedding, but in other portions of the range for instance, near the Jackson Forge, it assumes the character of a quartzose conglomerate, and exhibits distinct lines of bedding. A granite protrusion occurs along the line between townships 47 and 48, range 25, which has caused a displacement of the strata, many thousands of feet in a linear direction, throwing one portion of the ridge to the southwest, and another portion to the north; and it is instructive to observe how far the course of the Carp river has been determined by this dislocation. The main branch curves around the southern outlier, while the affluent, known as the Alder creek, finds its way between the granite and the quartz.

In section 31, township 48, range 25, near the west line, another band of compact limestone was observed, and thence traced westward, by Mr. Hill, through sections 33 and 36, being well exposed in the escarpments of the ridges. It is less silicious than that be-

fore described, variously colored, white, ash-grey and flesh-red, and beautifully veined with tints of a deeper hue. It calcines readily into lime, and affords beautiful ornamental materials.

Along the valley of the Carp, between Jackson Forge and Teal lake, beds of novaculite, or fine-grained silicious slate, are found interstratified with beds of quartz. It has been already quarried at several points, for hones, and there is, even now, a considerable demand for them. The beds are exceedingly fissile, and full of flaws at the surface, so that much of the mass is comparatively worthless; but it is believed that the blocks taken from a greater depth, and beyond the action of atmospheric agents, will be free from these imperfections. Messrs. Smith and Pratt have established a factory for the purpose of sawing these blocks, at the mouth of a small stream, near the Marquette landing, and are driving a thrifty business.

Between the quartz range and Dead river, the underlying rock consists, in the main, of chlorite and talcose slates, intersected by three belts of igneous rocks, ranging nearly east and west.

De la Beche, in reference to the greenstones and schistose rocks of Bossiney, Cornwall, remarks that, "there is so intimate a mixture of compact and schistose trappean rocks with the argillaceous that the whole may be regarded as one system, the two kinds of trappean rocks having been probably erupted, one in a state of igneous fusion, and the other in that of an ash, during the time that the mud, now forming slates, was deposited; the mixture being irregular from the irregular action of the respective causes which produced them; so that one may have been derived from igneous action, and the other from the ordinary abrasion of preëxisting solid rocks, and they were geologically contemporaneous."

This description is applicable to many of the igneous rocks of this region. They form neither long lines of dykes, nor axes of elevation, but broad sheets bearing the same relation to the slates that the trappean bands of Keeweenaw Point do to the conglomerates. Many of the slates appear to be composed of pulverulent greenstone, as though they might originally have been ejected as an ash, and subsequently deposited as a sediment, and they pass by imperceptible gradations, from a highly fissile to a highly compact state. In some places, for example, at Marquette and Presqu'isle, they assume a spheroidal structure, as though after their deposition they had been so far operated upon by heat as to allow a reärrangement of the particles. Along the lines of volcanic action, we frequently find, throughout this district, a green pulverulent substance, somewhat resembling chlorite, and containing a large amount of magnesia and lime, which is probably in the nature of an ash. The same ingredients enter largely into the composition of many of the trappean rocks, for they possess a soapy feel, and when reduced to a powder, effervesce feebly with acids.

There are undoubtedly, at this day, beneath the bed of the ocean, numerous "salses," which, from time to time, pour forth streams of pulverulent materials, but whose operation is concealed from human sight. We know that for weeks in succession, there flowed streams of chocolate-colored mud from the crater of Graham's island, before it finally sank below the surface. The contributions from this source, to the first formed stratified deposits, had not been duly appreciated. This volcanic mud is nothing more than the comminuted particles of trappean rocks, reduced by friction; and in the early history of our planet, when the fissures communicating with the interior were unfilled and volcanic energy was manifested more intensely than at this day, it would be reasonable to expect that igneous causes contributed as powerfully to the reduction of the preëxisting rocks, as the ordinary abrading action of water. The slates are composed essentially of the same ingredients as the trappean rocks with which they are associated, and the main difference between them may be, that the one was the product of salses, ejected in the form of mud, while the other was the product of volcanoes, ejected in molten streams. It has been supposed that the talcose nature of the slates associated with the igneous rocks was the result of metamorphism, but the supposition that they have resulted in some instances from the destruction of the latter, is quite as reasonable.

About a mile from the lake shore, on the road leading to the Jackson Forge, a low range of trappean rocks, of a compact texture, and of a dark-green color, is intersected. Between the Jackson and Marquette landings, by the lake shore, a similar belt is observed, which again appears near the junction of the roads, about four miles inland.

Another belt intersects the coast a short distance above the Marquette landing, which does not differ essentially from those before described. The slates in the vicinity of these belts are compact, of a greenish color, and traversed by different systems of joints, more distinct than the lines of bedding, cutting the mass into cuboidal blocks.

At Little Presqu'isle, another band of igneous rocks, of a highly crystalline character, projects into the lake. Distinct acicular crystals of hornblende are distributed in places through a paste of pure-white feldspar, while in others, these two minerals are disconnected, the latter forming beds of considerable thickness. Angular fragments of hornblende slate, chlorite slate; jasper and a green magnesian mineral, are seen enclosed in the mass near the water's edge which may be regarded as a volcanic breccia. These fragments seldom exceed a few inches in diameter.

Like most of the rocks of this region, its surface is smoothed and striated in a wonderful manner. Below the mouth of Dead

river, a highly crystalline mass of this character emerges in the form of an island fifty or sixty feet in height.

The main Presqu'isle consists of a dark-green trappean rock, rising in the overhanging cliffs to the height of a hundred feet. A description of this rock and the relations which it bears to the sandstone will be given when we come to treat of the Silurian system. Over this is deposited a volcanic tuff, imperfectly stratified, filling up the previous depressions, and attaining a thickness of twenty or thirty feet. It presents a complete net-work of veins, a few lines only in width, which penetrate but a short distance into the subjacent basalt. At one place, on the northwest side of the point an irregular vein bearing north and south is seen for two hundred feet in a linear direction, in this obscurely stratified tuff, which yields the sulphurets of lead, copper and iron, but not in sufficient quantities to render its exploration profitable. Asbestus is also sparingly distributed, and may be regarded as a metamorphic product resulting from the presence of lime. Traces of magnetic oxyd of iron are detected in some of the veins farther eastward.

Proceeding up the valley of Dead River, between sections 7 and 18, township 48, range 25, the stream is precipitated from a height of twenty feet over a ledge of schistose rocks, which exhibit distinct lines of bedding and abrupt convolutions of the strata.

In the next range west (27) the trappean and schistose rocks are frequently exposed in the bed of the stream, consisting of alternations of talcose and chlorite slates, and hornblende and feldspar rocks. They stretch out in numerous parallel ridges, bearing north of east and south of west, and present, for the most part, southerly escarpments. On the northwest quarter of section 16, the river is precipitated in a series of rapids over the former class of rocks, affording fine exposures for observation. On the west boundary of section 6, in a high ledge which rises from the northern bank of the stream, the slates are again observed dipping to the south at an angle of 70°.

The stream here bears west-northwest, conforming to the direction of the strata. After flowing along the northern line of township 48, nearly through range 27, it divides into numerous branches whose sources lie to the northwest, in the region of the granite.

Proceeding southward from Teal lake, we first encounter a ridge of trappean rocks which skirt its southern shore and rise abruptly to the height of two hundred feet above the lake-level, succeeded by chlorite slates and vast masses of specular and magnetic oxyd of iron. As we shall devote a special chapter to the character of these masses and their relations to the associated rocks, a more minute description is here deemed unnecessary.

We would merely observe that in this region the iron masses are invariably found in this association—never occurring in the granite.

These alternations of trappean and schistose rocks continue, to near the southern boundary of township 47, and are characterized in many places by the ores above described.

Section from Lake Superior to Lake Michigan.—The coast near the head of Keweenaw Bay (L'Anse) affords an admirable section of the slates and the overlying sandstone.

The following is the descending order of succession :

1 and 2. Fissile sandstone—the equivalent of the Potsdam—dipping slightly to the west-northwest, of a reddish color, and coarse-grained, passing into a conglomerate composed of pebbles of milk-white quartz, and occasionally trappean pebbles—13 feet, resting *unconformably* on the azoic rocks, consisting of

3. Chlorite slate and novaculite, or silicious slate, variously colored, and much contorted—in places folded over.

4. A dark hornblende and feldspar rock evidently trappean in its origin.

Formations 3 and 4 are traversed by veins of quartz which in no case penetrate the overlying sandstone. The slates are also occasionally intersected by dykes of trap.

This section is exceedingly instructive, inasmuch as it enables us to draw a line of demarcation between two formations different in age and external characters. While the newer formation—the Potsdam sandstone—is but slightly if at all disturbed and little changed by metamorphism, the older, or azoic slates, are contorted and folded into numerous arches, and in several places, invaded by igneous rocks. Their structure has been changed from granular to sub-crystalline, and the whole mass is intersected by numerous planes of lamination.

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Thickness of this System. Foldings of the Strata.—All attempts to estimate the thickness of the various schistose, calcareous and quartzose beds, must prove merely approximative. They occupy a belt which in its widest expansion reaches not less than eighty miles in width, and wherever exposed, have an inclination approaching verticality. If we were to deduct the spaces occupied by the purely igneous rocks, and then measure across the basset edges of those supposed to be sedimentary, the result would give us an incredible thickness—a thickness far surpassing that of the whole series of rocks heretofore observed, from the base of the Silurian to the crowning member of the tertiary. It is highly probable that the beds are arranged in a series of flexures, and that the observer in passing over the outcropping edges, beholds numerous repetitions of the same beds; they have, however, been so repeatedly shattered by earthquakes,

so disturbed and forced asunder by igneous protrusions, and so metamorphosed by direct and transmitted heat, that it is impossible to trace their continuity except over limited areas. If we could unfold these beds, and stretch them out in a nearly horizontal position, as when first deposited, they would require a far greater space than they now occupy. The causes by which these foldings have been effected, will be discussed in a subsequent chapter.

We have thus described the range, extent and mineral peculiarities of a series of rocks, detrital in their origin, interposed between the granite and the base of the Silurian system. Throughout their whole extent, they are more or less metamorphosed, presenting a series of gradations, represented at one extreme by crystalline gneiss and compact hornblende, and at the other by bedded limestone and ripple-marked quartz. To the presence of granite and trappean rocks this transformation is, in a great degree, to be attributed. Much of the compact hornblende presents the external characters of an igneous product; but, since it is found to occupy an almost invariable relation to the granite axis—flanking their slopes—and to assume a fissile structure as it recedes from the lines of igneous outburst, we cannot but regard it as the more highly metamorphosed portions of the dark-green chlorite slates. This compact hornblende is not to be confounded with those lenticular-shaped masses observed in the slates, which, we doubt not, are trappean in their nature.

We have seen that those igneous causes which produced numerous axes of elevation, and folded the strata into a series of flexures, had ceased to operate before the deposition of the Silurian groups, since they are found to repose in a nearly horizontal position upon the upturned edges of the slates, or to occupy the sinuosities in the granite, nowhere exhibiting traces of metamorphism or derangement of the strata. We do not now allude to the renewal of those igneous causes as manifested on Keweenaw Point and Isle Royale during the Silurian epoch, producing a class of igneous products widely different from those associated with the rocks of the azoic system. In a former report (Part I.) we have described the igneous rocks of the Silurian epoch as appearing under a variety of aspects, such as crystalline greenstone, porphyry, granular trap, and a highly cellular amygdaloid, differing little from modern lava, except that the cells are filled with various zeolitic minerals.

From the local details above given, it will be seen that the igneous rocks of the azoic period, though crystalline, compact, and occasionally porphyritic in their texture, are never amygdaloidal; and hence we infer that they were produced under widely different conditions. The latter may have been con-

solidated beneath the pressure of a deep ocean, while from the former a greater part of this pressure may have been removed; or it may be that both were, in the first instance, equally vesicular, but that the latter assumed a crystalline or compact structure from long-continued exposure to heat, under immense pressure. All the phenomena would seem to indicate that the eruption of the trappean rocks of this period took place beneath an ocean of great depth; or, at least, under conditions widely different from those which prevailed during the formation of the trappean belts of Keweenaw Point and Isle Royale.

Remarks.—The investigations of geologists in different parts of the world, within the last few years, have clearly demonstrated the existance of a series of non-fossiliferous rocks below the Silurian or Cambrian systems, and there can be no doubt that they are destined to occupy a conspicuous place in the classification of the rocks both of Europe and America. At the meeting of the American Association at Cincinnati, in the spring of 1851, we made the development of this system in the northern portion of the United States and Canada the special subject of a communication. Professor Mather, after having confirmed the accuracy of our views, from personal observation, stated that he had observed the continuation of this system near the sources of the Mississippi, and on the waters of the St. Peter's. Its existence in Missouri, where it is associated, as on Lake Superior, with immense beds of magnetic, and specular iron ore, is rendered certain by the observations of Mr. Mersh, which will be found incorporated in the subsequent pages of this report. At this meeting, Dr. King, who has examined this region with much care, confirmed these views, and we regret that the proceedings have not yet been made public, that we might quote his remarks in full.

Dr. Engelman, also, on that occasion, described a series of azoic rocks, as occurring in Arkansas, between Little Rock and the Hot Springs, which present a striking analogy with those of Missouri and Lake Superior, consisting of talcose, silicious and crystalline hornblende slates, often highly inclined, with beds of dark-blue limestone. On these older rocks, rests unconformably a sandstone, probably analogous to that of Lake Superior. Within this district of non-fossiliferous, stratified rocks, occurs a beautiful syenite. The vast masses of micaceous, or sub-magnetic oxyd of iron, which occur in Missouri, find their representative in the well known "Arkansas Magnets," or, in the iron ore of Magnet Cove. It is here associated with the interesting titaniferous minerals, schorlomite, brookite, and elæolite. Dr. E. thinks he has traced this series on the northern branches of the Colorado in Texas.

In the eastern portions of the United States, there can be no doubt of the existence of this system.

We are satisfied from personal observation that it flanks the Adirondack range in New York, where it is associated with hypersthene rocks and with masses of sub-magnetic oxyd of iron, below the Potsdam sandstone.

The Messieurs Rogers describe a series of obscurely stratified rocks in Pennsylvania and Virginia, occupying the same relative position known as the gneissoid series. They undoubtedly flank the Appalachian chain on the east, throughout their entire range, and will probably be found well developed in Tennessee and North Carolina.

In Europe the existense of this series has been established beyond controversy. It has been shown by eminent geologists, especially by Murchison and de Verneuil,* that the lowest beds in Scandinavia, containing the least traces of organic life, are the exact equivalents of the Lower Silurian strata of the British Isles, and that these have been distinctly formed out of, and rest upon, slaty and other rocks which had undergone crystallization before their particles were ground up and cemented together again to compose the earliest beds in which organic life is traceable. To this most ancient system of rocks in Scandinavia, they have given the name of azoic. By this term, they do not mean dogmatically to assert that nothing organic could have been in existence during the earliest times, when those rocks were in the process of formation, but simply to express the great fact, that, as far as our present state of knowledge goes, we look in vain for any traces of organic life, and it seems probable that they were formed under such physical conditions that nothing living could have flourished during that period.

The great mass of rock in Scandinavia is made up of a crystalline, granitic gneiss, presenting an almost infinite succession of feldspathic, quartzose, micaceous and hornblendic laminæ, which are often highly contorted, though a general strike or direction may be traced over a large tract of country. These rocks are by no means to be confounded with the metamorphic Silurian strata, occurring under a similar and analogous form in the same country. These azoic rocks are often disturbed and cut through by dykes of greenstone and traversed by countless veins of granite.

It is evident from the direct comparison of the more ancient azoic with newer metamorphic Silurian, that, from lithological characters alone, no distinction could be drawn between them, and it is only where the most conclusive evidence is afforded by superposition of the latter unconformably upon the former,

* Russia and the Ural Mountains, vol. i, p. 10.

that they can be clearly recognized and defined as belonging to different ages.

De la Beche remarks that, although alterations in the mineral character of the fossiliferous rocks, from the influence of intruded igneous matter in a molten state, or arising from other modifying causes, often produce mica slates, hornblende slates, gneiss and other forms of laminated and stratified deposits, with a peculiar aspect, there appears, nevertheless, evidence in Scandinavia and the British Isles, and also in other parts of Europe, to show that, beneath all the fossiliferous rocks, there are mica and chlorite slates, quartz rocks, crystalline limestones, gneiss, hornblende, and other rocks of earlier production. These may be, indeed, merely altered, or metamorphosed detrital and chemical deposits of earlier times, and *possibly* organic remains may eventually be discovered in them; but until this shall happen, it seems desirable to keep them asunder, for the convenience of showing previous accumulations to those known as the Cambrian group. He, therefore, proposes the name of *Mona Series* for the reception of these older rocks, which are well displayed in the island of Anglesea, in connection with those of the succeeding group.*

In the admirable and detailed examinations of the Geological Survey of Wales, where the numerous intercalated beds of trappean rocks and the complicated series of faults have rendered the task of unravelling the geology one of great difficulty, the surveyors have clearly shown the existence of this azoic series below the lowest Silurian strata, which is there represented by the sandstone of Barmouth and Harlech.

Barrande, also, in his investigations of the Bohemian basin, has recognized a series of semi-crystalline slates alternating with compact argillaceous slates, below the lowest Silurian strata, in which he has failed to detect any trace of organic life; hence he has applied the name azoic to these rocks without meaning to assert positively that the series is absolutely destitute of all traces of life, but simply as indicating the great fact that, thus far, none have been discovered.

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LOWER SILURIAN SYSTEM.

We now proceed to a description of the palæozoic series of this region. Unlike the rocks which we have hitherto described, they exhibit few traces of igneous outburst, and few displacements of the strata; but, on the other hand, they repose nearly horizontally on the basset edges of the slates, or occupy the depressions in the granite.

This general remark, however, must be received with some qualification, for we find that there existed, during a part of the

* Geological Observer, pp. 31, 32.

Silurian epoch, at least two lines of volcanic foci, from which flowed numerous streams of lava. These, mingling with the detrital deposits then in progress of accumulation, formed a mass, whose united thickness far surpasses the height of the loftiest summits in this region. We refer to the trap ranges of Keweenaw Point and Isle Royale, described in a previous report.

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The limits of this oceanic basin are but imperfectly defined. It stretched east and west, from the shores of the Atlantic to the flanks of the Rocky mountains; to the south, it extended beyond the Rio Grand, and north, to the Arctic ocean.

Within this basin, the granite axes between Lake Superior and Lake Michigan, and between Lake Superior and Hudson's Bay, rose above the waters of the Silurian sea.

A large portion of North America is embraced within this oceanic bed, constituting its fairest and most productive tracts. The uplifting force, by which this sea-bottom was converted into land, must have been gradually applied; since the strata, for the most part, repose in a nearly horizontal position, and exhibit few marks of derangement. We meet with no mountain chains, and no transverse valleys, except such as have been excavated by running water. The whole region is spread out in gently undulating plains; or, if ridges exist, they are due to accumulations of drift, or to the greater coherence between the particles in certain groups, which enabled them to resist the general denudation, which has everywhere left such incontestible evidence of its action.

From the MS. of Mr. Hall, we append some general remarks on the identity of the members of the Silurian groups, as developed in different parts of this basin.

"The observations of the past season have served to show that nearly all the more important groups of the Silurian system extend uninterruptedly from the more easterly points, where they have been investigated, through the northern peninsula of Michigan, as far west as the Mississippi, and even beyond. These observations form a connecting link between those heretofore made in New York and Canada, and those made in the southern peninsula of Michigan, Wisconsin, Iowa, Minnesota, and other portions of the West, and enable the geologist to form a correct idea of the range, extent, and fossil contents of these groups, as developed in the northern portion of the United States. We believe that these results will render some points of resemblance, heretofore obscure, clear and distinct; and remove any doubts that may have been entertained, as to the identity of certain members of the Silurian system, in their eastern and western prolongation. If these results are attained, we shall be satisfied that we have not labored in vain.

"These strata, originally deposited in a wide ocean, everywhere present evidences of organic existence. It is not to be supposed that in tracing deposits of this kind over so wide areas, the conditions of the ocean shores would have been uniform in their physical character, and we cannot, therefore, expect uniformity in the character of the deposits themselves. The very circumstances under which the sediment was thrown down and the causes which gave origin to it, are of themselves evidence, *a priori*, that absolute uniformity could not prevail over so wide an area. We are to look, in like manner, for corresponding changes in organic remains. It cannot be supposed that animals, possessing certain characters and habits of life, would continue to live for any length of time, when the physical conditions were unfavorable to their existence. When we consider, also, the extent over which these deposits have been traced, the difference in longitude alone would lead us to expect some differences in the fauna of this ancient period. We have only to compare this great linear development of the palæozoic strata with an equal extent of modern coast, to form some idea of the changes that might be expected to occur under similar circumstances. In making such a comparison, however, we shall find that the actual condition of these ancient deposits is far more uniform than that of the sea-shore, or sea-bottom, at the present day.

"By making the preliminary examination of recent shores, or of recent geological deposits, we are far better prepared to appreciate and understand the changes which will be observed in the rocks themselves. These changes, though gradual, and readily understood, when studied continuously, are, nevertheless, difficult of explanation, when seen at wide intervals, or examined at distant localities.

"The general results of these examinations, as will be seen by the local details, have shown an increasing thickness in the Potsdam sandstone, in its western extension, though, at some of the intermediate points, it is much thinner than in New York. It should be recollected, however, that these beds were deposited upon the uneven surface of preëxisting rocks, and their entire thickness may, therefore, be exposed only at some of the points where the originally unequal floor presents some of its elevations, and thus give an erroneous idea of its actual, entire thickness. The calciferous sandstone, in like manner, appears to increase as traced from the eastern limits of the district westward, and on the Mississippi, attains a thickness equal to that which it has in New York.

"From all of the evidence, it would appear that these two groups which are very intimately related to each other, have their most extreme tenuity somewhere near the northern boundary of the great arch, formed by the circuits of the older formations

around the northern shores of lakes Huron and Michigan. From this point, they increase in thickness as traced to the east and to the west. On the other hand, the lower limestones—the Chazy, Birds-eye, Black river, and Trenton groups—appear to decrease gradually in thickness, as traced westwardly from the Mohawk valley, and the outlet of Lake Ontario, where they exist in great force. This fact is made very conspicuous, when their entire thickness, as developed in New York, is compared with that of the same groups on the northern side of Lakes Huron and Michigan, where they contract to within a hundred feet, and in some places even less. We have also evidence that they do not, like the preceding groups, increase in thickness, when traced into Wisconsin and across the Mississippi; for there, they hardly attain a vertical range of fifty feet. Although over the entire area the identity of character and the continuity of the beds are maintained, it is clear that the materials have continually undergone diminution, and that the formation, unless it has been supplied from other sources, will be found to die out.

"In the upper member of the Lower Silurian system, the Hudson-river group of New York, we have also a striking example of the marked change which a deposit is found to have undergone when traced over a considerable area. In the district under consideration, it is clearly recognized throughout its whole extent, from Drummond's Island to Green Bay, except where it has been denuded, and the space occupied by lakes or bays. This group, like the other members of the limestone series before alluded to, exhibits a diminution in its thickness, as traced westwardly, and disappears, a short distance beyond the limits of the district.

"If we next consider the upper groups of the Silurian system, as displayed in this district, we find that, to a great extent, the reverse is true. Leaving out of view the Medina sandstone and the arenaceous portions of the Clinton group, which are scarcely recognizable here, we find the succeeding limestones much increased in thickness, and exhibiting no diminution, but rather an augmentation, as traced to the westward.

"From these general remarks, the reader will be prepared to understand the details given under each division of the series. It should be observed, however, in the outset, that the width of surface occupied on the map, by a particular group, is not always to be regarded as an indication of its thickness; for this, in most cases, is dependent upon the amount of dip in the beds, which, in this case, nearly corresponds with the slope of the country; so that it often happens that a group, less than a hundred feet in thickness, forms a belt several miles in width.

"The bearing and inclination of these successive groups indicate that they formed the outer margin of a great geological basin,

whose greatest depression is in the lower peninsula of Michigan, where the surface is occupied by rocks of the carboniferous epoch. It is only in a northern and northwestern direction, however, that we are enabled to trace the strata in a descending order quite to the lowest members of the series, and even to the non-fossiliferous series beneath them. In other directions, we find the most elevated portions of the border exposing only members of the upper, or at most, of the middle, portions of the Silurian system."

Potsdam Sandstone.—This was the first formed deposit in the Silurian basin, and attained, in its greatest development, a thickness of about two hundred and fifty feet, if we exclude the conglomerate-bands associated with the trap of Keweenaw Point and Isle Royale.

Range and Extent.—The bed of Lake Superior, embracing an area of about 32,000 square miles, is occupied almost exclusively by this rock, if we may judge from the islands which dot its surface, and the isolated patches which occur along its shores. On the north, the granite ranges approach near the coast and confine the sandstone within narrow limits; on the south, it occupies a broader area, and has been traced continuously around the axis, which divides the waters respectively flowing into Lake Superior, Lake Michigan, and the Mississippi. While the granite ranges attain in places an elevation of 1200 feet above the lake, the sandstones, except in the vicinity of the trap, do not reach higher that 350 feet, and rest in a position nearly horizontal. Consequently, the granites and slates rise up like islands through this great waste of sandstone.

This sandstone does not exhibit, at remote points, a homogeneity of character, or uniformity in thickness; but appears to have been modified, to a great extent, by local causes. Thus, in the vicinity of the trappean rocks, which afford ample evidence of intense and long-continued volcanic agency, the beds attain the enormous thickness of 5000 feet, and often consist of conglomerates, composed of trappean pebbles, cemented by a volcanic sand. Away from these lines of disturbance, and where it abuts against the azoic rocks, the mass consists of nearly pure silicious sand, enveloping pebbles of quartz and patches of slate. Where granite forms the adjoining rock, an equally marked change is observed in the character of the pebbles.

* * * * *

The Potsdam sandstone of New York is a quartzose rock, whose particles are firmly aggregated, while the same rock, on the northern slope of Lake Michigan, is so slightly coherent, that it may be crushed in the hand. The calciferous sandstone of New York, when traced west, passes into a magnesian lime-

stone. Even in that state, according to Hall, groups which, at one extremity, are of great importance, and well characterized by fossils, cannot be identified at the other.

The great mass of the materials which form the sandstones of this region appears to have been derived from the northwest, since the beds there attain a much greater thickness, and are composed of coarser sediments. They thin out as we trace them south and east, and are charged with fewer pebbles. On the Atlantic slope, according to the Messieurs Rogers, the sandstones expand in their southeasterly prolongation, while the limestones decrease. Hence, the ancient land was to the southeast and northwest, while the intervening space which formed the ocean bed, is occupied by the palæozoic series.

Sandstones of the Northern Shore. – On the northern shore of the lake, as before remarked, the sandstone occurs in insulated patches. According to Mr. Logan, to whom we are largely indebted for our knowledge of the geology of this portion of the region, on the isthmus between Thunder and Black Bays, sandstone occurs, composed of white silicious particles, but occasionally calcareous grains are intermingled. These beds attain a thickness of at least two hundred feet, and are succeeded by beds of red and white, in alternating layers. Conglomerates, containing pebbles of coarse red jasper, held in a reddish white or greenish sand, are also interstratified. The upper portion of the beds contains more calcareous matter, and some of the conglomerates envelope patches of limestone and chert. Mr. Logan estimates their entire thickness at five hundred feet. They are succeeded by limestones of a reddish-white color and very compact, interstratified with calcareo-argillaceous shales and reddish-white sandstones,—the whole attaining a thickness of eighty feet, with an addition of fifty feet of reddish, indurated marl. Succeeding these calcareous strata, after an interval, the amount of which is uncertain, red and white sandstones occur, with conglomerate layers, which become interstratified with trap layers, and an enormous amount of a volcanic overflow crowns the formation.

* * * * *

Sandstone of the Southern Shore.—Between Keweenaw Point and Isle Royale, there is, as we have before remarked, an immense curvature of the strata, probably reaching five hundred feet below the ocean level. A narrow belt of conglomerate and sandstone lines nearly the whole southern coast, from the head of Keweenaw Point to the Montreal river, beyond which, the latter, no longer associated with coarser bands, rapidly expands in width, and continues to Fond du Lac. For the most part, it appears in low ledges, or reefs, along the lake shore, but, at a few points, it rises in vertical cliffs, which afford many scenes of great beauty.

The bedded trap and conglomerate are admirably exhibited on Keweenaw Point, and in many places in the Ontonagon district; but as we have minutely described the geographical distribution of all the trappean rocks of this era with the associated conglomerate and sandstone, in a former report, a further description is deemed unnecessary.

* * * * *

[The features of this sandstone are described as presented at various points north of the lake, at Presqu'Isle, Carp River, Grand Island,—where the variety of colors the rock presents has given rise to the name of "Pictured Rocks,"—on the Menomonee River, White Rapids, etc.]

At the White Rapids, the sandstone is again exposed, presenting very nearly the same external characters, except that it is less discolored, and reposes on the uptilted edges of the quartz. It may be seen in some of the rapids below, and reäppears, for the last time, in the river banks, forming ledges six or eight feet high, about three miles above the Big Bend, in township 35, range 29.

"* In this vicinity, Mr. Desor discovered, in some of the loose masses of this rock, other fossils than the *Lingulæ*, which are so characteristic of this group further to the east. These fossils consist of the fragments of one or more species of trilobites, resembling *Asaphus*. From the characters preserved in a single caudal extremity, one species is identical with that which occurs in the same rock on the Missisippi and St. Croix rivers.

From the Menomonee river, the Potsdam sandstone approaches within fifteen or twenty miles of the shore of Green Bay, being distinctly exposed on all the streams flowing into it. Continuing in the same direction, its easterly limit passes near the Great Bend in the Wolf river, northwest from the outlet of Lake Winnebago. From thence, meandering westerly, it follows nearly the course of Wolf river, crossing it in the neighborhood of Lake Pauwaiceen, and is thence prolonged southwesterly towards Green and Puckaway lakes. In the neighborhood of Pleasant Valley, about twelve miles west of Strong's Landing, on the Fox river, it is exposed in several low escarpments, succeeded by the calciferous sandstone, which here presents its usual characters. From this region, its southern limit stretches to the west and northwest. The country here presents a feature which continues to the Mississippi river. The hills appear to be outliers, capped by the calciferous sandstone or succeeding limestones, while the valleys and the lower part of the escarpments are composed of the Potsdam beds.

* The description of the westerly prolongation of this sandstone is from the MS. of Mr. Hall.

The rock is fine grained, of a light yellow color and very friable. Some of the superior beds, which are thin, have been wrought for grindstones. The friable character of this sandstone is one of its most prominent features, and, owing to this circumstance, the escarpments are not usually high, or abrupt, unless it has been protected by the overlying rock. In its want of cohesion, it differs, in a very marked degree, from the prevailing character of this rock, as developed in New York and Canada, where it is usually, though not always, compact. It is not, however, unlike the sandstone of the Pictured Rocks, and is less friable than that of the Mississippi and St. Croix region.

The almost uninterrupted continuity with which this rock can be traced, even from its eastern extension through Canada and along the northern shore of Lake Huron to the St. Mary's river, and thence westerly, leaves no doubt as to its true position and identity in age with the Potsdam sandstone of New York. If we were at a loss in thus tracing it continuously, we have still the evidence of the succeeding fossiliferous strata, which show, conclusively, the same relations to this sandstone as they do to its equivalent in New York. With both these evidences combined, we cannot hesitate for a moment in our conclusion regarding its age and place in the series.

From the points just noticed, where this sandstone appears in eastern Wisconsin, it can be traced uninterruptedly across the entire breadth of the state to the Mississippi and St. Croix rivers. It is true, at the last named localities, we have the evidence of fossils which are not known to occur in its easterly extension; but we have already noticed the occurrence of the Trilobite on the Menomonee, while we have the Lingula everywhere, though in far greater profusion in the St. Croix region than elsewhere. In drawing inferences as to the age of the rock, from the occurrence of these fossils, it should be remembered that it is by no means improbable that similar ones may yet be found in more easterly localities. They seem to be coëxistent with calcareous bands, or the more calcareous portions of the group, and it is to this modification that we should look for the development of the fauna of this ancient period.

From all this evidence, we regard the question of the age of this rock as settled—that the Potsdam sandstone of New York is identical with that of the Mississippi and the St. Croix. One great source of doubt and perplexity in its determination, heretofore, was the recurrence of a sandstone identical in character with the lower, but superior in position to the calciferous sandstone, or lower magnesian limestone. It is a thin mass, evidently due to a recurrence of the same causes which produced the inferior deposit. This has been well elucidated by Dr. Owen in his reports on the upper Mississippi, in which he has shown that,

near the junction of the lower sandstone with the calciferous, there are several alternations of calcareous and silicious bands, the latter having the character of the sandstones below, and the former of the calcareous deposits above. These occur in several places on the upper Mississippi river, and give the geologist an introduction to that condition of things which subsequently produced the upper sandstone, which is distributed over a large part of Wisconsin, so often mistaken for the lower member of the series; but which, in fact, is separated from it by two or three hundred feet of calcareous rocks.

This upper sandstone can be regarded in no other light than as the result of the same causes which produced the Potsdam, and were suspended during the period of the deposition of the calciferous sandstone, or lower magnesian limestone, to be renewed, for a short period, in the deposition of a mass of sandstone, varying from fifty to eighty feet in thickness, upon the surface of the calcareous deposit. This fact shows the more intimate connection between these two lower groups than has heretofore been suspected. It is, nevertheless, shown in many places within the Lake Superior district, that the true sandstone, as it is traced upward, becomes gradually calcareous, and "finally passes into well-characterized, compact, magnesian limestone."* The same is true, also, of this rock, in Canada and New York; while, however, there is rarely any evidence of increase in the silicious materials towards the termination, as we observe in the west. In some localities, there are thin but distinct bands, near the upper portion, having an oolitic structure, which, as we go westward, appear to be replaced by beds of a granular texture and of a silicious character.

ART. III.—*Analysis of Tin Pyrites;* by Dr. J. W. MALLET.†

THIS rather rare mineral is one of which the chemical composition has appeared somewhat doubtful, owing to the considerable discrepancy between the three or four analyses which have been made of it, and to the fact that it almost invariably occurs massive and so intimately mixed with copper pyrites and other minerals as to render it difficult to select a fair specimen for examination. The locality from which most of the specimens in cabinets have been derived is Wheal Rock, near St. Agnes, Cornwall; but the mineral has also been observed in two or three other Cornish localities, and at Zinnwald, in Bohemia. I have recently received a specimen (name unknown) from a friend in England, which he states to have been found on St. Michael's Mount,

* Part 1, p. 117. † Communicated for this Journal.

Cornwall, and which, on examination, proves to be tin pyrites, and apparently in a purer state than any hitherto analyzed.

This specimen occurs in quartz which has obviously been taken from a vein in granite. The structure appears to be crystalline, although no distinct planes could be observed. The color is not steel-gray, as in that of the mineral from Wheal Rock, but iron-black, with slight superficial blue and red tarnish in some places. Streak black, lustre sub-metallic, fracture uneven. Hardness =4. Sp. gr. =4·522. Heated before the blowpipe, on charcoal, sulphurous acid is given off, oxyd of tin deposited in large quantity upon the charcoal, and a black globule obtained, from which copper and tin may be reduced on the addition of soda.

A carefully conducted quantitative analysis, in which chlorine was used to decompose the mineral, gave the following results.

			Equivalents.
Sulphur,	29·46	1·841	8·092
Tin,	26·85	·455	2·
Copper,	29·18	·920	4·044
Iron,	6·73 ·240	·464	2·040
Zinc,	7·26 ·224		
Gangue,	·16		
	99·64		

Thus the relative number of atoms of sulphur, tin, copper, and iron and zinc, as given in the 2nd and 3rd columns above, are almost exactly as 8 : 2 : 4 : 2; whence we have the formula first assigned by Kudernatsch, (Pogg. Ann., xxxix, 146,) viz., 2(Fe S+ZnS), SnS_2+2Cu_2S, SnS_2. The present analysis agrees so closely with this formula, from which the results of Kudernatsch, and even those of Rammelsberg, (Handw. d. Chem. Theils d. Mineral. 2d Suppl., 179,) sensibly differ, that it seems fairly to be considered as representing the composition of the mineral in a pure state. The analysis also possesses some interest in showing the presence of zinc in considerable quantity, therein agreeing with Rammelsberg's analysis above referred to of the mineral from Zinnwald. It is to be observed that in both cases the iron and zinc occur in very nearly atomic proportions, so that *perhaps* the formula should be written 2FeS, SnS_2+2ZnS, SnS_2+2(2 Cu_2S, SnS_2), though this does not seem very probable, since Kudernatsch found 12·44 p. ct. of iron to but 1·77 of zinc, while Johnston gives 10·113 p. c. of the latter to 4·791 of the former, as contained in tin pyrites (from the same locality as the specimen submitted to the present analysis, St. Michael's Mount). The presence of this large quantity of zinc, is however of importance, chiefly as proving that the tin must enter into the composition of the mineral as bi-sulphuret, since the other formula which has been proposed, namely, 2SnS, FeS_2 +2Cu_2S, FeS_2, wou'd

require us to admit the presence of ZnS_2, whereas no such compound is yet known either occurring in nature or formed by artificial means. It is remarkable that Fahlerz, the only other compound sulphuret in which zinc occurs in notable quantity, is the mineral whose composition ($4(RS)$, SbS_3) seems to approach most nearly to that of tin pyrites, the latter containing two instead of four atoms of the sulphur bases to one of sulphur acid, and this electro-negative sulphuret being bi-sulphuret of tin, instead of ter-sulphuret of antimony. Indeed, some real connection between these two minerals seems to be further indicated by their occurrence in the same crystalline system, and their close resemblance to each other in hardness, specific gravity, and general physical characters.

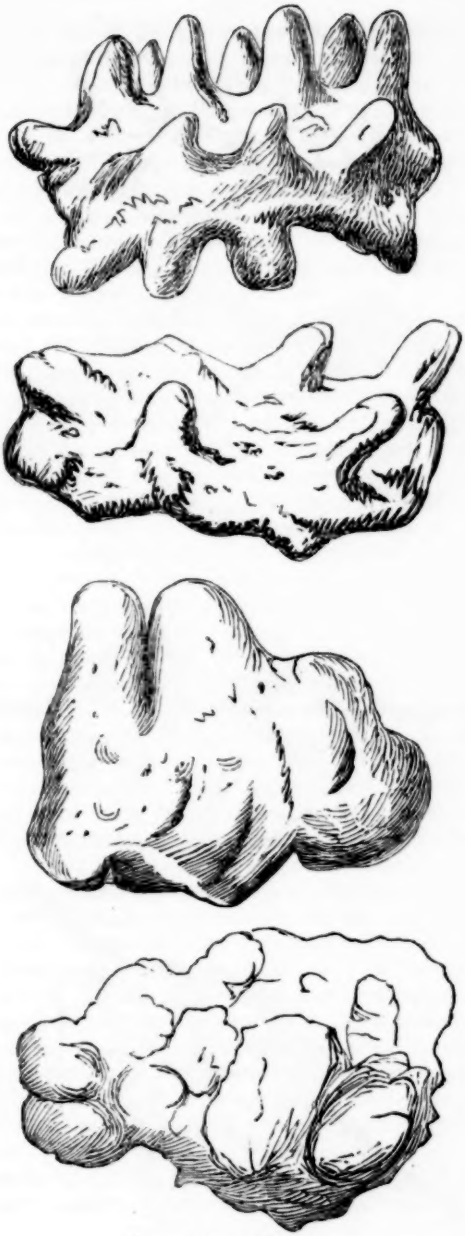
ART. IV.—*Notice of the Hail Storm which passed over New York City, on the first of July*, 1853; by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy, in the University of the City of New York.

ON the first of July, 1853, a very remarkable hail storm passed over the city of New York. The day had been uncommonly hot and sultry, the thermometer having risen to 90 degrees, and the air was believed to contain an unusual amount of vapor. A little before 5 o'clock in the afternoon, a heavy black cloud was observed to rise in the northwest, the wind at the time blowing moderately from the northeast, and subsequently from the east. As the cloud advanced and covered the northwestern sky, while it was still clear in the southeast, numerous streaks of zigzag lightning appeared to issue from the front margin of the cloud and descend towards the earth. I noticed the approach of the storm from my lodgings in Eighth street, within a quarter of a mile of the University. About five o'clock the wind came strong from the northwest, and the rain poured down in torrents. Presently I heard a loud thump upon the roof of the opposite house; soon another thump; and presently a third and fourth. I was not long in discovering that the noise was produced by the fall of hailstones of a size such as I had never before witnessed. They were few in number—but their average size was little less than that of a hen's egg; and one or two I am persuaded were fully as large as my fist. They almost invariably broke on striking the pavement; so that I could not secure either of those large stones except in fragments; and moreover the rain was falling in torrents. I however hastened to the yard in the rear of the house, hoping to find some upon the grass which had not been broken in the fall. After the rain had nearly subsided, we

found several handfuls of hailstones of good size, though altogether inferior to those which I saw in the street. They generally consisted of very transparent and solid ice, with many air bubbles; but they were not spongy in the centre as they are sometimes found. Hailstones sometimes occur which appear to be little more than pretty compact snow-balls. In the present instance, the hail was *not* of this kind. The large stones, however, generally consisted of an irregular assemblage of angular pieces of ice, which individually did not much exceed the size of hazel nuts—but they were cemented very firmly together. Indeed there was no appearance of seams or joints between these individual portions—but the ice was equally strong throughout every part of the mass. Their structure therefore did not indicate that several small hailstones were seperately formed and were subsequently cemented together; but rather that all were formed simultaneously about a common nucleus. Several persons independently, and without concert, suggested that the conglomerated mass resembled rock-candy: and the comparison appeared to me to be a very just one. There was a decided appearance of a tendency to crystallization. This tendency was in many cases towards a pyramidal form; in others they bore a resemblance to hexagonal prisms; and in some it appeared to me the tendency was towards a cubical form—though as the angles were all much rounded by the melting of the ice, I did not find any complete cubes.

Several of the stones which we picked up in the yard measured two inches long, and one measured over two and a half inches. These had been lying several minutes in a warm drenching rain; and it is my full conviction that two or three of those which I saw in the street were three and a half inches long, by two and a half inches wide, and they did not appear to deviate much from the spheroidal figure. A friend of mine, who is by profession a painter, and who saw and handled the hailstones, at my request made a sketch of some of the most remarkble of those which we picked up. These are shown in the accompanying figures which are drawn of the natural size. It is to be understood however that these were unquestionably smaller than many which we saw fall.

The rain, accompanied by thunder and lightning, continued for six or eight minutes, when its violence somewhat abated—it returned again with renewed energy, but soon afterwards entirely ceased. Another, but more moderate shower followed half an hour later, yet without either hail or lightning. Throughout the entire storm, the wind had blown with considerable force, but not with destructive violence, in that part of the city which is southwest of the University; and in the lower part of the city there was comparatively little wind.



Figures 1 to 4—Hailstones.

Figures 5 to 8—Hailstones.

In the upper part of the city, however, in the neighborhood of the Crystal Palace, the wind blew with destructive violence. A high brick wall was blown flat to the ground—a block of four wooden buildings (not entirely completed) was prostrated—and a small wing of the Crystal Palace was blown down. The fall of hail was heavy, and considerable glass in the Crystal Palace, and the buildings in the vicinity, were broken.

During the first part of the storm, the lightning was unusually severe. Several buildings and trees in New York and Williamsburgh were struck by the electric fluid, and one or two barns were burned to the ground.

I have succeeded in tracing this storm for a distance of full twenty five miles, and for about two-thirds of this distance have followed the track personally on foot. The portion of the track which I have myself surveyed, commences about a mile and a half southwest of Paterson, N. J., from which point it proceeds in a southeast direction—passing over the village of Acquackanonck, together with the cities of N. York and Williamsburg—and from this point the storm can be traced with diminished energy to Jamaica Bay. Near Paterson, the wind is believed to have been more violent than in any other part of the above mentioned track. Where it swept through the forests, many large trees, of one to two feet in diameter, were overturned—while others were snapped off and twisted like reeds. This remark applies to a distance of about three or four miles from the commencement near Paterson. In the neighborhood of Acquackanonck, a few trees were overturned—but not a large number. East of Acquackanonck, the track soon crossed the Hackensack meadows where the ground is low and flat, and there were no trees to be overturned. I have obtained no information of the effect of the wind upon the high ridge on the west bank of the Hudson river—but the entire length of the track across New York was marked by violence, as above stated. This region was particularly exposed since it was the highest ground encountered by the storm in its passage across the island. Having crossed East river, the storm passed centrally over Williamsburgh, where it caused more damage than in any other part of its course. The steeples of two churches (the first Presbyterian and the Dutch) were blown down; the roof of a third church was partially blown off—the roofs of a large number of dwelling houses were carried away—the tin from numerous roofs was rolled up in long solid coils, and in some cases carried off—while in others, it clung to the roofs and was left after the storm in long massive windrows.

The breadth of the track near Paterson is thought not to have exceeded half a mile—perhaps was somewhat less than this—and

the destructive violence did not extend beyond these limits, until the storm reached Williamsburgh: but here the wind was almost equally violent over a space a mile and a half in breadth, while houses were unroofed over a track two miles in breadth. Beyond Williamsburgh, the wind was less destructive—the track became broader and less distinctly defined—and the general course deviated more to the east. The storm was reported as severe at Jamaica and South Jamaica. From the commencment near Paterson to Williamsburgh the track did not deviate sensibly from a straight line; and its course was from N. 40° W. to S. 40° E.

Throughout the entire track here mentioned, hail fell of unusual size. Near Paterson the stones were smallest in size, but most abundant in quantity. The destruction caused to the fruit and the crops was such as not unfrequently occurs in France, but has seldom been witnessed in this country. When I visited the spot a few days after the storm, the trees looked as if they had been pelted by myriads of heavy stones. The leaves were strewed thick upon the ground; and most of those which still clung to the branches, were riddled through and through, and dried upon the stems. The rails of the fences bore marks of large gashes where the brown weather-worn surface had been nicked off and a fresh surface exposed, as if by a volley of stones from a troop of mischievous boys. Upon the north side of the houses along the track, scarce a pane of glass was left entire, and the clapboards were covered thick with gashes an inch in diameter, where the paint was chipped off. Fields of wheat and rye, which had not been harvested, were beaten down as flat as if a heavy iron roller (such as is sometimes employed for smoothing gravelled walks) had been dragged over them; and fields of corn were totally destroyed. On some fields, I was assured that after the storm the ice lay in a solid compact mass two inches thick. Large quantities of ice still remained unmelted on the ground the next morning, and a tenant on one of the farms collected a considerable quantity and carried it into Paterson, (two miles distant,) to show to his landlord; and I was informed that in a hollow, against the side of a house, the ice accumulated to such a depth, that on the evening of July 2nd, the day after the storm, a bushel basket full of ice was shoveled up; the stones varied in size from that of a pigeon's egg, to a hen's egg. The track of the ice did not deviate much from the track of greatest violence of the wind, and followed the same general direction, but covered a somewhat greater breadth. From Paterson to the Hackensack meadows beyond Acquackanonck, the damage caused by the hail was very great, amounting to nearly an entire destruction of the crops of wheat, rye, oats and corn, as also the cherries, peaches, apples, etc., within the limits of the track.

In the city of New York, the damage done by the hail was not very great; for the stones were not numerous, although of prodigious size. The ship-yard of Mr. Thomas Collyer at the Dry Dock, was covered with singularly shaped pieces of ice,—one of which was measured and found to be 6¼ inches in circumference —another seven inches, and a third measured three inches long, and two inches thick.

In Williamsburgh the hail appears to have destroyed more glass than in New York. In many houses nearly half the glass was broken in windows which were unprotected on the north side. Over 400 panes of glass were broken from the north side of a single school house.

On the same day with the preceding storm, large hail is said to have fallen at several places in Pennsylvania. About three o'clock in the afternoon, a terrific hail-storm passed over Northumberland doing great damage. Hail-stones are said to have been picked up measuring 7¾ inches in circumference; and several thousand panes of glass were broken in that town.

At 5½ o'clock, P. M., a severe hail-storm passed about 20 miles north of Philadelphia. At Upper Dublin, the storm was very destructive. Several barns were unroofed, many fruit and forest trees were blown down—and many fields of wheat and oats so badly damaged, as scarcely to pay for harvesting. One hailstone was measured, and its greatest circumference found to be 6½ inches, and its smallest five inches,—and this was half an hour after the storm had abated. At Norristown and Doylestown the crops were much injured by the hail, and at Burlington, N. J., the wind was exceedingly violent.

It is not probable that either of these storms was the same as that which passed over New York. The hail-storm near Philadelphia, was about simultaneous with that at New York. The storm at Northumberland may have been identical with that at Upper Dublin, the distance of the places being 100 miles—the interval of time 2½ hours—and the direction nearly parallel with the track of the New York storm.

It would appear that a violent wave of great extent set in from the northwest, which rolled over both New York and Philadelphia, and within this wave were formed about simultaneously several distinct veins of hail.

Was the storm which passed over New York a whirlwind?

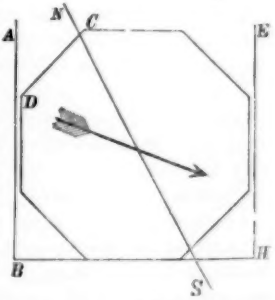
I have surveyed every part of the track of the storm where I have heard of any violent effects, especially with reference to the decision of this question. Throughout Williamsburgh, I could find no unequivocal evidence of rotation. The steeples which were prostrated, fell in a direction coinciding very nearly with that of the storm's progress, that is, towards the southeast. In the case of one of the churches whose steeple was blown down

(the first Presbyterian Church) I noticed a phenomenon which I considered worth recording. The track crossed the ridge of the church at an angle of about 45°. On the leeward side, the tin roof was started from the boards (but not broken) and puffed up forming a wrinkle about twenty feet long, two or three feet wide, and ten inches high. This appears to me to indicate the operation of a force beneath, pushing up the tin; but not being able to tear the tin open, bulged it up and left it in a ridge.

This phenomenon appears to be analogous to what often occurs in tornadoes, and I ascribe it to a rarefaction of the air on the leeward roof. A current of air, forcibly impelled over an obstacle like the roof of a building, by friction drags along with it the air lying upon the leeward side of the roof, producing a partial rarefaction, and the air beneath by its expansion, tends to lift the roof. Thus the leeward roof is often carried away, while the windward roof remains. In the present case, this upward pressure lifted the tin about ten inches, stretching but not tearing it. This force appeared to be insufficient to tear the tin from its fastenings—perhaps because from the carrying away of the steeple, and the ripping up of the adjacent edge of the tin, the air beneath found a ready escape.

In the neighborhood of the Crystal Palace, occurred a phenomenon which appeared to indicate the existence of currents blowing nearly in opposite directions. The wooden buildings, which have already been mentioned, were blown toward the southeast —but the brick wall, the line of which run from N. 28° E. to S. 28° W., fell toward *the west;* that is, in a direction nearly contrary to that of the storm's progress.

The following appears to me to be the explanation of this phenomenon. The Latting Observatory is an octagonal tower, 300 feet high and 75 in diameter at base, sloping uniformly to the top. In the annexed figure, the octagon represents the base of the tower, and the line N S represents a meridian. On the west side of the tower, was erected a brick wall A B, 25 feet high, and only three feet from the side of the tower. At the south end, it was connected with another wall B H, but at the north end it was entirely free, and had no support except an iron clamp projecting from the side of the tower. The direction of the storm's progress is indicated by the arrow. It might have been anticipated that the wall A B would have been thrown towards the east upon the tower; whereas in fact it was thrown outwards towards the west. But we know



from the testimony of spectators, that this wall fell at the first onset of the blast, when, as we shall presently see, the wind blew nearly from the north, or perhaps a little east of north. Now a violent current from the north, driven into the triangular space A D C, would necessarily become condensed between the wall and the tower, exerting a force to push the wall outward, and the wall had little strength to resist the pressure, being weak not only from its great height, but also being unsupported at the north end. The bricks also had been recently laid, and the mortar was not yet dry. On the east side of the tower, was a similar wall E H, but only 14 feet high, which was not prostrated. Its security is probably to be ascribed to its inferior height.

The following facts at first seemed to me a little puzzling. Near Paterson, a large oak limb, a foot thick, was twisted off at the height of fifty feet, and prostrated in a direction pointing S. 20° E., the top of the limb lying towards the base of the tree. Within a short distance I found another large oak limb, torn off at a great height and thrown towards S. 40° E., with its top also turned towards the base of the tree. Not far off, I found a third limb in a similar position. Broken limbs were generally found to have been carried eastward, with the top pointing to the S. E., and the base towards the N. W. The three cases I have here specified were exceptions to the general rule, and it appears to me that they are to be explained by supposing that the branches turned a somerset in falling.

A like case occurred with the steeple of the first Presbyterian Church in Williamsburgh. The spire fell across the street—the top struck a brick house on the opposite side of the street and broke off, while the upper portion of the spire remained imbedded in the roof of the house which was crushed in by the blow. The remainder of the spire, which was now the frustum of a pyramid, fell down into the street; but it is probable that the smaller end of the frustum struck the pavement first, for the steeple turned a somerset along the street towards the east, and lay after the storm with the smaller end of the frustum turned *towards* the church.

A similar supposition will satisfactorily account for the observed position of the three limbs above mentioned.

In the woods between Acquackanonck and Paterson, I measured with a pocket compass the direction of a large number of prostrate trees. The following list shows the entire range of the bearings which I measured; *not* arranged in the order in which they were measured, but classified according to the points of the compass. They were

S. 70° W.; S. 20° W.; S. 15° W.; S. 10° W.; south; S. 10° E.; S. 20° E.; S. 25° E.; S. 30° E.; S. 35° E.; S. 40° E.; S. 45° E.; S. 50° E.; S. 60° E.; S. 70° E.; east.

These bearings were measured at various points upon a portion of the track about two miles in length; and it will be noticed that there is not a single instance of a tree which was prostrated towards any point between East, North and West. The bearings extend from east, through south, to S. 70° W., a range of 160 degrees; but I found only one instance of a bearing approaching nearly so close to the west point. With but one exception, the bearings were all confined between east and S. 20° W. The wind did not then blow from every point of the compass indifferently, at least not with sufficient force to prostrate trees, but it blew only from the northward, including northeast and northwest. Neither was the wind a simple rectilinear current. What law then did the wind observe? Was its motion merely centripetal? Did it revolve in a whirl? Or did it follow some other law?

In order to decide these questions, I attempted to apply the method which I had successfully practised in the Mayfield tornado of Feb., 1842. This method consisted in selecting groups of prostrate trees which lay upon each other, and measuring successively the bearings of the bottom tree, of the next in order, and so on to the top, and regarding these bearings as indicating the successive directions of the wind at the point of observation, as the storm passed by. At Mayfield I had no difficulty in finding groups of trees piled upon each other, frequently four or five in a group; but at Paterson I found in no case more than two trees crossing at a considerable angle, and only five instances of this description. The following are the observations in the cases referred to, the bearing first mentioned in each case being that of the bottom tree.

1. { S. 50° E. / S. 20 E. } 2. { S. 40° E. / S. 25 E. } 3. { S. 20° E. / S. 40 E. } 4. { S. 40° E. / S. 10 E. } 5. { S. 70° W. / S. 60 E. }

The first four cases present no angle greater than 30°; the fifth case presents an angle of 130°; that is, the two trees were turned in nearly opposite directions.

From a comparison of all the facts, I conclude that the wind blew first from the northeast, and that this current was succeeded by a north and presently a northwest wind. The following are my reasons for this conclusion.

1. The fifth case of interfering trees, just mentioned, taken in connection with all the bearings observed, points to this conclusion. We find that one large tree was prostrated with its top turned towards a point S. 70° W. Upon it lay another large tree with its top turned S. 60° E. We may safely infer that these directions corresponded nearly with the direction of the wind when the trees were prostrated, and that the wind came first from a point N. 70° E., and was succeeded by a current from N. 60° W. In each of the other cases of interfering trees, the angle of crossing was so small, as to convey no very distinct information

on this question. In three cases out of the four, the first blast appears to have been a little more westerly than the final one; but all the trees were prostrated by a northwest wind.

2. A very intelligent farmer, whose house was close upon the northeast margin of the track, about four miles from Paterson, gave the following testimony. He first took refuge from the hail under a shed on the southwest side of his barn, the wind then blowing from the N. E. After a short time, the hail began to beat upon him, the wind having veered to the N. W., and he was obliged to seek a shelter on the southeast side of his barn in order to escape the hail.

3. It is known from the testimony of several individuals, that the wind at New York was easterly on the first approach of the storm.

Upon comparing these facts, it appears to me that the direction of the wind at the time of its most destructive violence may be tolerably well represented by the annexed diagram, showing a

current from the N. E., on the front of the storm; and from the N. W. in the rear, the whole having a progressive motion towards the S. E., which would give to each place in succession (unless near the southwest margin of the track) first a N. E. wind, and afterwards a N. W. wind.

I do not then find in this case that evidence of a complete rotation which I have found in some other tornadoes; but it is possible that at a little elevation above the earth's surface, the rotary motion may have been more decided.

What was the cause of the hail?

The hail was caused by a violent upward movement of the air, carrying along with it an unusual amount of vapor, which was suddenly condensed, and at so low a temperature, that it was frozen in large semi-crystalline masses.

That there was a violent upward movement of the air, I infer from the following considerations.

1. Rev. J. W. McLane of Williamsburgh was in the street near his house, and noticed the coming up of the storm. He says the cloud was very dense and black—moved rapidly forward—and under the main sheet, the clouds boiled up in a violent and angry manner, which led him to anticipate a severe blast. Other observers have testified to substantially the same facts.

2. It appears impossible that two currents, in close juxta-position, should blow from nearly opposite quarters, with sufficient violence to prostrate large trees, unless there is opportunity for the air to escape by an upward movement. This conclusion is also in perfect harmony with what we have frequent occasion to observe in small sand whirls and water spouts.

How was the cold which formed the hail produced?

According to the observations of Pouillet, in France, the temperature of hail-stones when they fall, is sometimes as low as 25° Fahr. They must then have been formed at a temperature considerably below that of melting ice—a temperature probably as low as 20° Fahr. How can so low a temperature be produced in the hottest weather of July? The temperature of the air diminishes as we ascend from the earth, and at the height of 8800 feet above New York, is estimated at 32° in summer. At the height of 12000 feet the temperature is reduced to 20°. Were the hail stones in the present case, formed at an elevation of 12000 feet? It does not appear to me that we are at liberty to make such an assumption. In the summer of 1835, several hail-storms passed over the southern part of France, where there were insulated peaks of mountains, which afforded precise means of measuring the elevation of the hail. In the storm of July 28th, 1835, no hail fell on the summit of the Puy du Dome, an elevation of 4800 feet above the sea; but a few stones fell at the height of 3700 feet, while at the foot of the mountain, the ground was covered to the depth of three inches, and some of the stones weighed eight ounces. On the 2d of August of the same year, a hail cloud enveloped the summit of the mountain, rising therefore to the height of at least 5000 feet.

It does not therefore appear to me that we are at liberty to assume that the hail of July 1st, was formed at an elevation much exceeding 5000 feet, and here the summer temperature may be estimated at 46°. This cold is of course insufficient to form ice.

It is believed that during the passage of a hail storm, the temperature of the upper air is considerably below the mean. The simple presence of clouds in the lower atmosphere would tend to produce such an effect. The atmosphere derives its heat from the earth, and is but little affected by the direct passage of the solar rays. The heat which the earth imbibes from the sun is continually thrown off by radiation;—but when the surface of the earth is covered by a cloud, this radiant heat is intercepted, and the temperature of the lower air is thereby elevated. On a still night, the presence of clouds sometimes causes the thermometer to stand ten or fifteen degrees higher that it would otherwise. But if, by the interposition of a cloud, the lower atmosphere becomes unusually hot, the atmosphere above the cloud must receive less than its usual supply of heat, that is, it must become unusually cold.

Moreover, in the storm of July 1st, the hail was formed in a current blowing violently from the northwest, which came therefore from a higher latitude, and of course brought with it a diminished temperature. I have no data sufficiently precise for estimating the effect to be ascribed to this cause, but I think we may conclude that at the time of the storm in question, at an elevation of 5000 feet above New York, the temperature could not have differed much from 32°. We have not however yet reached the temperature necessary to the production of hail.

Another source of cold is to be found in the evaporation from the surface of the hailstones. It is well known that if we tie a piece of thin muslin upon the bulb of a thermometer, and then after dipping the bulb in water, swing it rapidly through the air, the thermometer will sink below the temperature of the air, several degrees, sometimes ten or fifteen; an effect due to evaporation. During a hail storm, the hot air from the earth's surface is carried by the upward movement to a considerable elevation, by expansion it is cooled, and a portion of its vapor is condensed. The drops thus formed at a temperature not far from 32°, are still further cooled by evaporation from their surface, (the evaporation being promoted by their rapid motion;) the remainder of the drop is congealed; and as new vapor is precipitated, it is congealed upon the former;—thus forming concentric layers round the nucleus. Since water, like nearly every other substance, in passing to the solid state, inclines to crystallization, the ball as it increases, does not generally retain the spherical form, but shoots out irregular prisms.

How does a hailstone remain suspended in the air long enough to acquire a weight of half a pound?

This difficulty is not, to my mind, a very formidable one. I conceive that hail stones are formed with great rapidity. The vapor is condensed with great suddenness and almost instantly frozen. I think very large hailstones may be formed in five

minutes. In a vacuum, a stone would fall from the height of 5000 feet in less than 20 seconds—but drops of water and hail stones fall with only a moderate velocity. From my own observations of the hail stones of July 1st, I estimated the velocity of their fall at about 40 feet per second. At the uniform rate of 40 feet per second, a stone would be more than two minutes in falling 5000 feet.

In order to obtain some reliable data for estimating the velocity of hail stones, I have computed the greatest velocity of a number of small bodies differing in size and specific gravity. Dr. Hutton determined by numerous experiments the resistance of the air to bodies moving with different velocities; and in the third volume of his Tracts, p. 218, has given a table of the air's resistance to a sphere 2 inches in diameter. His experiments also indicated that the resistance, of spheres increases in a ratio somewhat greater than the squares of the diameters. This excess for spheres of from 2 to 4 inches diameter was about $\frac{1}{30}$th part of the resistance. The second column in the following Table is taken from Hutton's Tracts, the resistances for velocities from 50 to 100 being supplied by interpolation. The resistance for a sphere 1 inch in diameter is found by taking one-fourth of the numbers in the second column, and diminishing the result by one-thirtieth part. Each succeeding column is derived from the preceding in a similar manner.

Resistance of the air to Spheres of different Diameters.

Velocity per second	Sphere 2 inches in diameter.	Sphere 1 inch in diameter.	Sphere $\frac{1}{2}$ inch in diameter.	Sphere $\frac{1}{4}$ inch in diameter.	Sphere $\frac{1}{8}$ inch in diameter.
feet.	ounces.	ounces.	ounces.	ounces.	ounces.
5	0·006	0·001	0·000	0·0001	0·0000
10	0·026	0·006	0·001	0·0004	0·0001
15	0·058	0·014	0·003	0·0008	0·0002
20	0·103	0·025	0·006	0·0015	0·0004
25	0·163	0·039	0·010	0·0023	0·0006
30	0·237	0·057	0·014	0·0033	0·0008
35	0·325	0·078	0·019	0·0046	0·0011
40	0·427	0·103	0·025	0·0060	0·0015
45	0·544	0·131	0·032	0.0077	0·0019
50	0·676	0·163	0·039	0·0095	0·0023
55	0·821	0·198	0·048	0·0116	0·0028
60	0·981	0·237	0·057	0·0138	0·0033
65	1·155	0·279	0·067	0·0163	0·0039
70	1·343	0·325	0·078	0·0190	0·0046
75	1·546	0·374	0·090	0·0218	0·0053
80	1·764	0·426	0·103	0·0249	0·0060
85	1·996	0·482	0·116	0·0282	0·0068
90	2·243	0·542	0·131	0·0317	0·0076
95	2·505	0·605	0·146	0·0354	0·0085
100	2·780	0·672	0·162	0·0392	0·0095
200	11·340	2·764	0·668	0·1615	0·0390
300	25·800	6·235	1·507	0·3641	0·0880

In a vacuum, a body falling under the influence of gravity is continually accelerated; but when a heavy body falls through the atmosphere, the resistance increases with the velocity, until the resistance becomes equal to the weight of the body. When this takes place, there can be no further increase of velocity, and the body will afterwards descend with a uniform motion. In order therefore to determine the greatest velocity which a heavy body can acquire by falling through the atmosphere, it is only necessary to compute the weight of a sphere of given diameter, and then to search in the preceding table for the velocity due to an equal resistance upon a body of the proposed diameter. The following Table exhibits the results for spheres of lead (assuming the specific gravity 11·35), of iron (specific gravity 7·78), of water, of ice (sp. gr. 0·93), and cork (sp. gr. 0·25); the diameters varying from two inches to one-eighth of an inch.

	Weight of a sphere of					Final velocity of sphere of				
Diam.	Lead. ounces.	Iron. ounces.	Water. ounces.	Ice. ounces.	Cork. ounces	Lead. feet.	Iron. feet.	Water. feet.	Ice. feet.	Cork feet.
2 in.	27·5132	18·8593	2·4241	2·2544	0·6060	310	257	94	90	47
1 "	3·4392	2·3574	0·3030	0·2818	0·0757	223	185	68	65	34
½ "	0·4299	0·2947	0·0379	0·0352	0·0095	161	134	49	47	25
¼ "	0·0537	0·0368	0·0047	0·0044	0·0012	117	97	36	35	18
⅛ "	0·0067	0·0046	0·0006	0·0006	0·0001	84	70	25	24	12

Thus it appears that a hail stone in the form of a sphere two inches in diameter, falling through a tranquil atmosphere cannot possibly acquire a velocity exceeding 90 feet per second, and spheres of a smaller size would acquire a still less velocity. Also a hail stone of irregular shape would experience more resistance than a sphere, and would acquire a smaller velocity in falling. An upward current of air moving with a velocity of 90 feet per second, or 61 miles per hour, would sustain a sphere of ice two inches in diameter; also an upward current of 30 miles per hour would sustain hail stones of half an inch in diameter, and would greatly reduce the velocity of stones of larger size. The strong upward movement which is known to exist in the neighborhood where hail is formed, is therefore quite sufficient to sustain hail stones of the largest kind as long as they can be kept within the influence of this vortex. After they have entirely escaped from the influence of the vortex, small stones would fall to the earth from an elevation of 5000 feet in about two minutes; and very large stones in one minute. I see no difficulty therefore in supposing the great mass of hail to remain in the air five minutes before reaching the earth—and that in peculiar cases, stones may remain supported for ten minutes and even a great deal longer. This period appears to me sufficient to account for the hail which fell at New York.

Why did the hail in the present case attain to such unusual size?

Because of the following circumstances which are unusually favorable to its formation. The temperature of the air before the storm was 90°, and it is my opinion that the dew-point could not have been less than 80°; in other words the atmosphere contained about as much vapor as it is ever known to contain in this latitude. This vapor was suddenly lifted to a region of great cold, and rapidly condensed and frozen. The strong upward movement helped to sustain the crystals as they increased in size, until the upward force was no longer equal to gravity—or until they escaped from the influence of the vortex. Most of the stones would fall in five minutes and be of moderate size; others might be sustained ten or fifteen minutes, and attain enormous dimensions.

How did the hail in this storm compare with the most remarkable cases on record.

There are well authenticated cases of hailstones having fallen in England and France weighing half a pound—and even more than this—but the accounts of hail stones weighing so much as one pound, do not appear to me entirely satisfactory. A mass of ice of the specific gravity 0·93, weighing eight ounces must contain nearly 15 cubic inches; or would make a cube whose edge is nearly 2·5 inches. I have selected a piece of ice which was estimated to be about the size of the largest stone which I saw fall on the 1st of July, and found it to weigh eight ounces. But these large stones of July 1st appeared to me unusually white, and may therefore be conjectured to have had a spongy nucleus—which would have reduced the weight to perhaps six ounces.

The hail therefore in the present storm was somewhat smaller than has been observed to fall in other places.

Since the preceding was written, I have received notice of several remarkable hail storms in different parts of the United States, two of which were so extraordinary that I have added an account of them to this paper.

Hail Storm experienced at Warren, N. H., Aug. 13, 1851.

My first information respecting this storm was derived from a letter from Dr. Peter L. Hoyt, dated Wentworth, Grafton Co., N. H., Aug. 3, 1853. The following is an extract from Dr. Hoyt's letter.

"Perhaps a brief notice of a hail storm which occurred in this vicinity on the 13th of August, 1851 may be of interest to you. This shower, about one o'clock, P. M., passed from the west towards the east over an extent of four or five miles around the base of Moosehillock Mountain, in the towns of Benton and Warren. The largest and most hail fell in the north east part of

the latter town, in a basin between the mountains near the source of a stream called Baker's river. I stood at the railroad depot in Wentworth, at the time of this shower, distant in an air line six or seven miles. It was the most remarkable appearing cloud I think I ever saw—so black and dense, encircling and covering the mountain, and shutting down to the earth.

"The hail was of prodigious size and in great quantities. The largest of the stones was of an irregular shape, rough and angular, suggesting the idea to some that they were made up of a number adhering together. They were however very solid and not easily broken.

"One was weighed upon the spot at the time of the shower, and weighed 20 ounces; and the person who informed me of this was of the opinion that he saw one fall and break in pieces which was still larger. They looked, he said, like vast pieces of ice that had been broken above, and were falling to the earth. A quantity was gathered in a basket and brought to Warren village, a distance of three or four miles, and there exhibited at least an hour after the shower, and in a hot and sultry afternoon. One of them there weighed 14 ounces, and measured 10 inches in circumference. Twelve of the largest taken out of the basket weighed on the counter scales in the store, seven pounds.

"About 4 o'clock, P. M., three hours after their fall, a box of them was brought to Wentworth village, where I reside, a distance of about eight miles. One of them was shown to me. Its diameter according to my best judgment was from 2 to 2½ inches. It had the appearance of being originally somewhat angular, with the angles melted off. It was perfectly solid and clear.

"So large was the quantity of hail in many places in Warren, that a cart load might have been gathered without moving from the place. Luckily the track of the storm was not through the most cultivated part of the towns, but along the borders and skirts of the forests, where the population was scattering. Crops of hay and grain were ground to the earth, poultry were killed—cattle's backs were bruised—and the roofs of many buildings were badly broken. But little glass was broken from the fact that the direction of the hail was nearly perpendicular to the earth."

Immediately upon receiving this letter, I wrote to Dr. Hoyt, stating that the facts which he had communicated respecting the size of the hail were so remarkable, that they ought to be substantiated by such evidence as would be deemed conclusive in a court of justice; that it was therefore important that he should obtain written statements from the identical persons who weighed the stones; and that it would not be derogatory to the dignity of science for these persons to make affidavit to the truth of their statements before a Justice of the Peace. I also suggested several

additional points upon which it was desirable that information should be obtained. In reply I received another letter from Dr. Hoyt accompanied by documents such as I had suggested. The following is extracted from his reply.

"As yet I have been unable to substantiate the weight of a hail-stone at 20 ounces; yet throughout the town of Warren the impression prevails that one was so weighed. The enclosed affidavit of Mr. Libby, and statement of Mr. Flanders fixes the extreme weight of two stones weighed by them at 17¾ and 18 ounces; with the firm belief of Mr. Libby that had he weighed them at the time of falling, their weight would have been increased some two or three ounces.

"I have the names of some two or three other individuals who weighed several stones of a pound weight,—others were weighed weighing three fourths of a pound. One several hours after the storm weighed 14 ounces. A tin pail full containing fifteen, weighed 10 pounds—four collectively weighed three pounds, etc. Incredible as the above may appear to some, they are facts which can be proved by a multitude of evidence.

"This storm was remarkable for the amount of ice which fell as well as the size of the stones. Mr. Flanders thinks that in Benton the average depth of the hail was about four inches, and from enquiry along the track of the storm, I should judge that he is not far from right in his estimate. The extreme width of the hail was about two miles, and the length over a cultivated district perhaps about five or six miles. How far east it extended I have no means of knowing, as it entered a forest of many miles in width. The largest hail stones fell near the edge or skirts of its track; the thickest and greatest amount or depth of hail fell in the centre. Although the sun came out "boiling hot" as one man expressed it, after the shower, still the hail remained on the ground in many places until the next day. An owner of a saw mill, on the stream of water which has its source in the forests over which the shower passed, told me that the water kept swollen for two or three days, when from common showers of rain it would have fallen in twenty-four hours. This he attributed to the gradual melting of so large a quantity of ice in the woods. I think there was but little if any difference in the distribution of the large stones along the track, as the two whose weights are given by Mr. Flanders and Mr. Libby were picked up about five miles apart, and near the extremes of its track before it entered the forest. On the borders or skirts of the cloud the large stones fell scattering, and as it approached the centre it was as if the whole contents of the cloud were let down in ice. During the time of the hail, which lasted some twenty or thirty minutes, there was but little rain; after the hail it rained briskly for ten or fifteen minutes.

"*Shape of the hail.*—In Benton at the commencement of the hail, the masses were angular, having a resemblance to broken ice; while further along the track they assumed a smoother and more uniform surface, being oval or oblong. In many instances the surface is described as being notched or scolloped; and in some few as being covered with icy spikes, like icicles somewhat resembling a burr. It is the opinion of those who examined these stones the most minutely, that they were not formed by the union of several masses, but were distinct and individual in formation. They were compact and very solid; so hard that they might be thrown with great force against a house and not be broken.

"*Velocity.*—All agree that the hail fell with great velocity and force. Mr. E. W. Cleasby, a very correct and veracious man, whose statement is appended to this letter, says that hail stones very solid and weighing in the vicinity of 10 or 12 ounces, averaging one on about every two feet, fell in a piece of unmowed grass. In their passage through the grass they entangled it so as to carry it imbedded into the sward ground to the depth of some two or three inches, and after the melting of the hail, left the turf full of holes like little bird's nests. These holes remained through the season. As a test of the force necessary to effect this, he repeatedly with a pitch fork handle having a rounded head, tried to strike it into the ground to an equal depth, and was unable to do it.

"Many of the barns in this neighborhood have their roofs covered with what are styled 'long shingles'—that is with spruce shingles without previous boarding. Whenever these large stones fell upon such roofs, they broke a hole completely through; and one man having sought refuge in a barn under such circumstances, was obliged to hide under the scaffold. The marks and bruises upon the buildings caused by the hail are still to be seen. Says one person, 'they looked like little pumpkins falling.' The roar and rattle of the hail was distinctly heard at the village in Warren, a distance of four or five miles, and was likened to the noise of a heavy train of cars.

"*Wind.*—During the storm there was but little wind. The hail fell nearly perpendicularly. The general bearing of the wind as appears from my weather table on that day, was west-south-west; and the direction of the shower was in correspondence with this.

"*Heat.*—No record of the heat was kept in the neighborhood of the storm. My thermometer at 2 o'clock, P. M., indicated 76 degrees. I very well recollect that after witnessing the passage of this cloud to the north, the sun broke out very hot and scorching. Such also is the testimony of people living there.

"You ask if it was possible that the larger stones could have been formed by the cementing or freezing together of several while lying on the ground? I should think it *impossible* that such could be the case. Furthermore the general opinion among the inhabitants is that each stone was a unit in formation."

The following is a copy of the affidavit of Mr. Libby already referred to.

Warren, N. H., Aug. 24, 1853.

I live in Warren and witnessed the hail storm on the 13th of August 1851, between the hours of one and two o'clock, P. M. I weighed a number of the hail stones which fell at that time, but not until after the shower had ceased—perhaps an hour and a half or two hours after. During this time it was very hot. The largest which I weighed was 17¼ ounces in weight. The others varied in the vicinity of a pound. I am fully in the belief that had they been weighed at the time of falling, their weight would have been some two or three ounces more. Previous to weighing them I washed the dirt from them in water. They were very irregular in shape, somewhat scolloped, with ice projections from their surface. I picked these stones up from soft ploughed ground where they were imbedded more than half their size in the ground. During the time that the hail was falling there was but little rain, with little or no wind. After the hail there was a warm rain of some ten or fifteen minutes duration. JOHN LIBBY.

Sworn to before me, JESSE LITTLE, *Justice of the Peace.*

The following is the statement of Mr. Flanders already referred to.

Wentworth, Aug. 30, 1853.

I live in Benton, N. H., County of Grafton, and resided there at the time of the hail storm on the 13th of August, 1851. I weighed a number of the hail stones after the rain was over. The heaviest one weighed 18 ounces; the others ranged in the vicinity of a pound. They were very irregular in shape—some nearly square—some scolloped—some angular as if made up of several pieces. According to my best judgment there was an average depth of four inches of hail which fell at that time.

GRANVILLE E. FLANDERS.

The following is the statement of Mr. Cleasby.

Warren, N. H., Sept. 3, 1853.

This certifies that several of the hail stones which fell here on the 13th of August, 1851, were measured by members of my family. According to my best recollection the circumference of the largest was fourteen inches one way and nine the other. Their form was very generally oval. EZRA W. CLEASBY.

The preceding evidence satisfies me that hail stones fell in New Hampshire on the 13th of August, 1851, weighing *more than one pound;* and I do not know of any satisfactory evidence that hail of equal size has ever been seen in any other part of the world.

Hail Storm at Montrose, Iowa, on the Mississippi River, in Lat. 40° 30′, *about the middle of June,* 1838.

The following notice of this storm is derived from a letter received from Mr. D. W. Kilbourne, who resided at Montrose in 1838, but now lives at Keokuk, twelve miles below Montrose.

"About four o'clock in the afternoon, a very heavy black cloud rose in the northwest, the wind at the time blowing strong from that quarter. There was much thunder and lightning; at the same time it was clear in the east and southeast.

Very soon however the whole sky seemed to be covered by clouds; there was a heavy mist, and it was almost as dark as night. Rain immediately followed, and for a few moments fell in torrents. Then hail stones began to fall. At first they were small and excited no surprise in myself or family; but they continued to increase in quantity and size to such an extent as to excite not only our wonder but our fears. The hail storm continued nearly ten minutes, and during all the time small and large hail fell together. The wind was high.

As soon as the storm abated so that it was safe to go out, my family were all engaged in picking up the stones. We then selected the largest and measured their circumference. The largest one found *measured ten and one-fourth inches.* There were a large number that measured from two to ten inches in circumference. I gathered up with my own hands in one spot on the grass without moving, a half bushel measure full.

Mrs. Kilbourne placed several of the largest ones upon the top of common sized glass tumblers, and when melted they filled the tumblers so that some of them could not be moved without spilling the water.

The hailstones were irregular in their formation, and presented very much the appearance of rock-candy. The ice was solid and transparent. We did not weigh them.

The hail fell only about half a mile in width, and not more than two miles in length from west to east. No hail fell on the east side of the river. But few white families resided in the neighborhood at that time, or in the county; and I do not believe the hail stones were particularly noticed or measured except at our house."

ART. V.—*Description of a Tertiary Rainbow;* by CHARLES HARTWELL.*

ON the 28th of July, 1851, the writer observed, from the Theological Seminary, in South Windsor, Conn., what he judged to be a Tertiary Rainbow. After a heavy shower, and a little before sunset, the sun appeared, painting on the dark clouds in the east a beautiful primary bow. At the same time an appearance of decomposed light was seen in the N. W., upon a cloud of not very large dimensions, but from which rain was evidently falling. To the S. W., also, upon clouds somewhat separated, decomposed light was visible.

The appearance north of the sun was very bright, though in it were observed only the various shades of red and orange. It extended, according to my judgment, a degree or more in horizontal width, and from five to ten degrees upward. To the south the phenomenon was less brilliant, less in width, but distinctly traceable for some fifteen degrees from the horizon. Had these phenomena appeared in the east, no one would have doubted but that they constituted the two ends of a rainbow. The curvature of the colored light, and the correspondence in position, would have been sufficient proof. But as they were seen in the west, on the side with the sun, and tertiary bows are very rarely seen, it may be necessary to give the reasons which convinced me that I had really seen one. The phenomenon to the north was first observed, and filled the beholder with astonishment. What this appearance could be, so much more brilliant than ordinary views of the sun's shining on clouds, and then, too, not on the edge but near the middle while the rest appeared as clouds ordinarily do, at the same time no reason being manifest from the position of the cloud and sun and the state of the intermediate heavens why the sun should shine on that part rather than another, not a little puzzled him.

On going to another window, the phenomenon to the south was seen. From its greater length, curved form, and its position on the opposite side of the sun, the conclusion was immediately drawn that they were the two ends of a rainbow. Recalling some instructions of my former teacher, Prof. Snell, of Amherst

* *Messrs. Editors.*—The accompanying paper was prepared by Rev. Charles Hartwell, now connected with the mission in China, to be read at the meeting of the American Association in Albany, 1851. Being unable to attend himself, he sent it by a friend, who was, however, obliged to leave before the day assigned for its reading. Ill-health and pressing engagements prevented his giving further attention to it, till he was on his voyage to China; when, lighting upon the notes, he transmitted them to me, to be disposed of as I should think best. Though more than two years have elapsed since the phenomenon herein described was observed, I think the account of it ought, even now, to find a place in some public record.

Yours truly, E. S. SNELL.

Amherst College, Nov. 11th, 1853.

College, the thought flashed into my mind that this was a tertiary bow. Not recalling the dimensions of such a bow, I measured off the heavens as best I could, and judged the radius as seen to be about 40°. I have since learned that the radius by calculation is 40° 40′, so that my judgment, correct or incorrect, agrees very well with the true dimensions of the bow.

Having stated these facts to Prof. Snell, and requested a brief communication from him, he kindly furnished one, which I append to this paper.

"Amherst College, Aug. 15, 1851.

Since hearing you give an account of a phenomenon which you observed and supposed to be a tertiary rainbow, I have examined my notes upon the subject, and am well satisfied that you were not mistaken. All the circumstances forbid the supposition that it was a halo formed in prisms of ice. You estimated the distance of the arcs from the sun to be 40°; this differs but about half a degree from the radius of the tertiary bow, as determined by calculation. The arcs were seen also in masses of falling rain of such limited thickness, that the light might well be supposed to have been transmitted through the rain to the eye.

Though the appearance was not one of remarkable splendor, yet you may congratulate yourself, I think, on having witnessed a phenomenon of the most rare occurrence. The writer of the treatise on Optics, in the English Library of Useful Knowledge, speaking of the bows caused by *three or four* reflections in each drop of rain, says, 'none of these bows, however, have been seen.' Professor Forbes, of the University of Edinburgh, in his learned Report on Meteorology to the British Association, 1840, remarks, 'These, the ternary, quaternary, &c., rainbows, have been long theoretically known, though rarely, if ever, observed in nature. The ternary rainbow ought to be about 41° from the sun, but is generally stated to be too faint to be visible. Two observations by Bergmann are the only recorded ones I have met with. Kæmptz observed a ternary amidst the spray of the falls of Schauffhausen.'

Kæmptz, in his course of meteorology, after speaking of the bows of the third and fourth order, adds, 'but the intensity of these two latter is so feeble that they are rarely seen;' and puts in a note the following quotation, 'M. Babinet, when in the most favorable circumstances, on Mount d'Or, and on the Canigou, vainly endeavored to perceive them.'

It is obvious, as these quotations show, that the phenomenon which you witnessed is as rare as it is interesting. It seems to me highly probable that there is yet no public record of an instance in which an American observer has seen a rainbow on the same side of the heavens with the sun."

ART. VI.—*The Earl of Rosse's Telescopes, and their Revelations in the Sidereal Heavens;* by REV. W. SCORESBY, D.D., F.R.SS.L. & E., etc.*

IN a second lecture on these interesting subjects, recently delivered at Torquay, much and important consideration was given to the inquiry,—What has the gigantic telescope done?

The lecturer having himself had the privilege of observing on different visits, and for considerable periods, with both the instruments, was enabled to reply, he hoped in a satisfactory manner, to this inquiry. His opportunities of observing, he said, notwithstanding interruptions from clouds and disturbed atmosphere, had been somewhat numerous, and, not unfrequently, highly instructive and delightful. Of these observations he had made records of nearly 60, on the moon, planets, double stars, clusters, and nebulæ. He had been permitted also to have free access to, and examination of, all the observatory records and drawings, so that he was enabled on the best grounds, he believed, to say, that there had been no disappointment in the performance of the instruments; and that the great instrument, in its peculiar qualities of superiority, possesses a marvellous power in collecting light and penetrating into regions of previously untouched space. In what may be called the domestic regions of our planet—the objects in the solar system—all that other instruments may reveal is within its grasp or more, though by the prodigious flood of light from the brighter planets, the eye is dazzled unless a large portion is shut out.

But in its application to the distant heavens and exploration of the nebulous systems there, its peculiar powers have, with a steady atmosphere, their highest developments and noblest triumphs. In this department—that to which the instrument has been particularly directed—every known object it touches, when the air is favorable, is, as a general fact, exhibited under some new aspect. It pierces into the indefinite or diffuse nebulous forms shewn by other instruments in a general manner, and either exhibits configurations altogether unimagined, or resolves perhaps the nebulous patches of light into clusters of stars. Guided in the general researches by the works of the talented and laborious Herschels—to whom astronomy and science owe a deep debt of gratitude—time has been economized, and the interests of the results vastly enhanced. So that many objects in which the fine instruments of other observers could discern only some vague indefinite patch of light, have been brought out in striking, definite, and marvellous configurations.

Among these peculiar revelations is that of the *spiral* form—the most striking and appreciable of all—which we may venture to designate "*The Rossean Configuration.*" Its discovery was

* An abstract of a Lecture delivered at Torquay, November 15, 1852.—From the Edinburgh New Philosophical Journal for January, 1853.

at once novel and splendid; and in reference to the dynamical principles on which these vast aggregations of remote suns are whirled about within their respective systems and sustained against interferences, promises to be of the greatest importance.

One of the most splendid nebulæ of this class—the *great spiral* or *whirlpool*—has been figured in the Philosophical Transactions for 1850. It may be considered as the grand type and example of a class; for near 40 more, with spiral characteristics, have been observed, and about 20 of them carefully figured. Dr. Scoresby had the pleasure of being present at the discovery of this particular form in a nebula of the planetary denomination, in which two portions following spiral forms were detected. Its color was peculiar,—pale blue. He had the privilege, too, of being present on another interesting occasion, when the examination of the great nebula in Orion was first seen to yield decisive tokens of resolution.

In these departments of research, the examination of the configurations of nebulæ, and the resolution of nebulæ into stars, the six-feet speculum has had its grandest triumphs, and the noble artificer and observer the highest rewards of his talents and enterprise. Altogether, the quantity of work done, during a period of about seven years,—including a winter when a noble philanthropy for a starving population absorbed the keenest interests of science,—has been decidedly great, and the new knowledge acquired, concerning the handiwork of the Great Creator, amply satisfying of even sanguine anticipations.

Dr. Scoresby found, in September last, that about 700 catalogued nebulæ had been already examined, and transferred to the ledger records from the journals of the Observatory, (comprising only a selection from the general observations,) and the *new nebulæ*, or nebulous knots, discovered merely incidentally, amounted to 140 or more. The number of observations, involving separate sets of the instrument, recorded in the ledger, (exclusive of very many hundreds, possibly thousands, on the moon and planets,) amount to near 1700, involving several hundreds of determinations of position and angular measurements with the micrometer on the far distant stars. The carefully drawn configurations, eliciting *new characteristics*, exceed 90, and the rough or less-finished sketches amount to above 200. Of the 700 catalogued nebulæ already examined, it should be observed, that in full one-half or more, something new has been elicited.

In speaking of the effects of the flood of light accumulated by the six-feet speculum of the Earl of Rosse, Dr. Scoresby remarked, that this peculiarity of the instrument (connected as it is with due length of focus and admirable definition) enabled it to reach distances in space far beyond the powers of any other instrument. This was its peculiar province; and in this, as to existing instruments, there was not, nor, as he hoped to shew, could there be,

any competition. For comparing the space-penetrating power of the six-feet speculum with one of two feet (which has rarely been exceeded) we find it three to one in favor of the largest, with an accumulation of *light* in the ratio of 6^2 to 2^2, or 9 to 1. On comparing the powers of this magnificent instrument with those of a refractor of two feet aperture, the largest hitherto attempted, we have a *superiority*—making a due allowance for the loss of light by reflection from two mirrors, and assuming an equal degree of perfectness, figure, and other optical requirements in the refractor, and *no* allowance for absorption of light—in the ratio of about 4·5 to 1, as to light, and as 2·12 to 1, as to the capability of penetrating space, or detecting nebulous or sidereal objects at the extreme distance of visibility. Hence, whilst the range of telescopic vision in a refractor of two feet aperture would embrace a *sphere* in space represented by a diameter of 2; the six-feet speculum (assuming both instruments to be of equal optical perfection, magnifying equally, and allowing fifty per cent. for loss of light for two reflections in the one case, and none (?) in the other) would comprehend a sphere of about 4·24 diameter,—the outer shell of which, 1·12 in thickness, being the province of the great instrument alone. But let us reduce these proportions to *sections* of equal spaces, that we may judge more accurately of the relative powers. Now, the solid contents of different spheres, we know, are in the ratio of the cubes of their diameters. Hence the comparative spheres, penetrated by the two instruments re-referred to, should be $4{\cdot}24^3$ to 2^3; that is, as 9·5 to 1. Deducting, then, from this vast grasp of space the inner sphere, capable of being explored by other instruments, we find that, out of nearly ten sections of space reached by this telescope, there are nearly nine sections which the six feet speculum may embrace as peculiarly its own!

What its revelations yet may prove, then, we can have no idea. Several thousands of nebulæ have been catalogued: the great reflector might add to these tens of thousands more. But this, seeing how few nights in a year are favorable for the highest powers, must be the work of years of perseverance. It would be a worthy undertaking for the government of a great country, to afford the means of multiplying such gigantic instruments. Application is to be made, in this direction, for a six-feet reflector at the Cape of Good Hope, for the examination of the heavens towards the southern pole. Lord Rosse, with his usual nobleness of liberality, will yield up his laboratory, machinery, and men, to the service of government, and is willing, moreover, to give the direction and guidance of his master-mind. Will the British nation be content with a refusal?

The range opened to us by the great telescope at Birr Castle, is best, perhaps, apprehended by the now usual measurement—not of distances in miles, or millions of miles, or diameters of the

earth's orbit, but—of the progress of light in free space. The determination, within, no doubt, a small proportion of error, of the parallax of a considerable number of the fixed stars, yields, according to M. Peters, a space betwixt us and the fixed stars of the smallest magnitude, the sixth, ordinarily visible to the naked eye, of 130 years in the flight of light. This information enables us, on the principles of *sounding the heavens*, suggested by Sir W. Herschel, with the photometrical researches on the stars of Dr. Wollaston and others, to carry the estimation of distances, and that by no means on vague assumption, to the limits of space opened out by the most effective telescopes. And from the guidance thus afforded us, as to the comparative power of the six feet speculum in the penetration of space, as already elucidated, we might fairly assume the fact, that if any other telescope now in use could follow the sun if removed to the remotest visible position, or till its light would require 10,000 years to reach us, the grand instrument at Parsonstown would follow it so far, that from 20,000 to 25,000 years would be spent in the transmission of its light to the earth. But in the cases of clusters of stars, and of nebulæ exhibiting a mere speck of misty luminosity, from the combined light perhaps of hundreds of thousands of suns, the *penetration* into space, compared with the results of ordinary vision, must be enormous; so that it would not be difficult to shew the *probability* that a million of years, in flight of light, would be requisite, in regard to the most distant, to trace the enormous interval.

But after all, what is all this, vast as the attainment may seem, in the exploration of the extent of the works of the Almighty? For in this attempt to look into space, as the great reflector enables us, we see but a *mere speck*—for SPACE IS INFINITE. Could we take, therefore, not the tardy wings of the morning, with the speed of the mere spread of day, nor flee as with the leaden wings of light, which would require years to reach the nearest star, but, like unhampered *thought*, could we speed to the farthest visible nebula at a bound,—there, doubtless, we should have a continuance of revelations; and if bound after bound were taken, and new spheres of space for ten thousand repetitions explored, should we not probably find each additional sphere of telescopic vision garnished with suns and nebulous configurations rich and marvellous as our own? If these views serve to enlarge our conception of creative wonders, and of the glory and power of the Great Architect of the heavens, should they not deeply impress us in respect to the Divine condescension in regarding so graciously this little, inferior world of ours! Animated with the spirit of the Psalmist, we shall each one, surely, be disposed appropriately to join in his emphatic saying,—"When I *consider* thy heavens, the work of thy fingers, the moon and the stars which thou hast ordained; what is man, that thou art mindful of him? or the son of man, that thou visitest him?"

Art. VII.—*Researches on the Development of Viviparous Aphides;* by Waldo I. Burnett, M.D., Boston.

Every naturalist is aware of the remarkable phenomena connected with the viviparous reproduction of Aphides or plant-lice, for their singularity has led them to be recounted in works other than those of natural science, and, from the days of the earlier observers, they have been the theme of a kind of wonder-story in zoology and physiology.

I need not here go over the historical relations of this subject. The queer experiments and the amusing writings of the old Entomologists are well known. The brief history of the general conditions of the development of these insects is as follows: In the early autumn the colonies of plant-lice are composed of both male and female individuals; these pair, the males then die, and the females soon begin to deposit their eggs, after which they die also. Early in the ensuing spring, as soon as the sap begins to flow, these eggs are hatched, and the young lice immediately begin to pump up sap from the tender leaves and shoots, increase rapidly in size, and in a short time come to maturity. In this state it is found that the whole brood, without a single exception, consists solely of females, or rather and more properly, of individuals which are capable of reproducing their kind. This reproduction takes place by a viviparous generation, there being formed in the individuals in question, young lice which, when capable of entering upon individual life, escape from their progenitor and form a new and greatly increased colony. This second generation pursues the same course as the first, the individuals of which it is composed being like those of the first, sexless, or at least without any trace of the male sex throughout. These same conditions are then repeated, and so on almost indefinitely, experiments having shown that this power of reproduction under such circumstances may be exercised, according to Bonnet,* at least through nine generations, while Duvau† obtained thus, eleven generations in seven months, his experiments being curtailed at this stage, not by a failure of the reproductive power, but by the approach of winter which killed his specimens; and Kyber‡ even observed that a colony of *Aphis dianthi* which had been brought into a constantly heated room, continued to propagate for four years, in this manner, without the intervention of males, and even in this instance it remains to be proved how much longer these phenomena might have been continued.

The singularity of these results led to much incredulity as to their authenticity, and on this account the experiments were often

* *Bonnet,* Traité d'Insectologie, ou observations sur les Pucerons, 1745.
† *Duvau,* Mém. du Mus. d'Hist. Nat., xiii, p. 126.
‡ *Kyber, Germar's* Magaz. d. Entomol., 1812.

and carefully repeated; and there can now be no doubt that the virgin *Aphis* reproduces her kind—a phenomenon which may be continued almost indefinitely, ending finally in the appearance of individuals of distinct male and female sex, which lay the foundation of new colonies in the manner just described.*

The question arises, what interpretation is to be put upon these almost anomalous phenomena? Many explanations have been offered by various naturalists and physiologists, but most of them have been as unsatisfactory as they have been forced, and were admissible only by the acceptance in physiology of quite new features.

As the criticism I intend to offer upon some of these opinions, will be the better understood after the detail of my own researches, I will reserve their future notice until the concluding part of this paper.

My observations were made upon one of the largest species of *Aphis* with which I am acquainted, the *Aphis Caryæ* of *Harris*.† While in Georgia, this last spring, it was my good fortune that myriads of these destroyers appeared on a hickory which grew near the house in which I lived. The number of broods on this tree did not exceed three, for with the third series their numbers were so great that their source of subsistence failed and they gradually disappeared from starvation. The individuals of each brood were, throughout, of the producing kind, no males having been found upon the closest search; they were all, moreover, winged; and those few which were seen without these appendages appeared to have lost them by accident. I mention this fact especially, since it has been supposed by naturalists that the females were always wingless, and therefore that the winged individuals, or the males, appeared only in the autumn.‡

The first brood, upon their appearance from their winter hiding-places, were of mature size, and I found in them the developing germs of the second brood quite far advanced. On this account it was the embryology of the third series or brood alone, that I was able to trace in these observations.

* For details of experiments by which *Bonnet's* original results were verified, see *Réaumur*, Mémoires, iii, Mém. 9 and 11, and vi, Mém. 13. Also,
Degeer, Mémoires, iii, ch. 2, 3.
Curtis, Trans. Linn. Soc., vi. Philos. Trans. 1771.
Sauvages, Journ. de Physique, i.
Dutrochet, Mémoires, ii, p. 442. See also the more modern writers, and especially *Kirby* and *Spence*, Introduction to Entomology, iv. p. 161.

† *Harris*, A treatise on some of the Insects of New England which are injurious to Vegetation. 2nd ed. 1852. p. 208. As *Dr. Harris* says, it is probably *Lachnus* of Illiger. (*Cinara* of Curtis.)

‡ See *Westwood*, An introduction to the modern Classification of Insects, &c. London, 1839. ii, p. 438—but especially
Owen, Parthenogenesis, &c., p. 23, note, and p. 59, note, where he says, "Many of the virgin viviparous Aphides acquire wings, but never perfect the generative organs!"

A few days after the appearance of these insects, the individuals of second brood (B), still within their parents (A), had reached two-thirds of their mature size. At this time the arches of the segments of the embryo had begun to close on the back, and the various external appendages of the insect to appear prominently; the alimentary canal had been more or less completely formed, although distinct abdominal organs of any kind belonging to the digestive system were not very prominent. At this period, and while the individuals of generation B, are not only in the abdomen of their parent A, but are also enclosed, each, in its primitive egg-like capsule,—at this time, I repeat, appear the first traces of the germs of the third brood (C).

These first traces consist of small egg-like bodies arranged, two, three, or four in a row, and attached in the abdomen at the locality where the ovaries are situated in the oviparous forms of these animals.

These egg-like bodies consisted either of single nucleated cells, of $\frac{1}{3\,0\,0\,0}$ of an inch in diameter, or, a small number of such cells enclosed in a simple sac. These are the germs of the third generation; they increase with the development of the embryo in which they have been formed, and this increase of size takes place not by a segmentation of the primitive cells, but by the endogenous formation of new cells. After this increase has gone on for a certain time, these egg-like bodies appear like little oval bags of cells—all these component cells being of the same size and shape, there being no cell which is larger and more prominent than the rest, and which could be comparable to a germinative vesicle. While these germs are thus constituted, the formation of new ones is continually taking place. This occurs by a kind of constriction-process of the first germs, one of their ends being pinched off, as it were, and in this way what was a single sac, is changed into two which are attached in a moniliform manner. The new germ thus formed, may consist of even a single cell only as I have often seen, but it (the germ) soon attains a more uniform size by the endogenous formation of new cells within the sac by which it is enclosed. In this way the germs are multiplied to a considerable number, the nutritive material for their growth being apparently a fatty liquid with which they are bathed, contained in the abdomen, and which is thence derived from the abdomen of the first parent.

When these germs have reached the size of $\frac{1}{3\,0\,0}$ of an inch in diameter, there appears on each, near one end, a yellowish, vitellus-looking mass or spot, which is composed of large, yellowish cells, which in size and general aspect, are different from those constituting the germ proper. This yellow mass increases *pari passu* with the germ, and at last lies like a cloud over and concealing one of its poles. I would also insist on the point that it

does not extend itself gradually over the whole germ-mass, and is therefore quite unlike a true germinative vesicle or a proligerous disc. When the egg-like germs have attained the size of $\frac{1}{125}$ of an inch, there distinctly appears the sketching or marking out of the future animal. This sketching consists at first of delicately-marked retreatings of the cells here and there, but which soon become more prominent from furrows, and at last the whole form of the embryo stands boldly out. As the whole idea and form of the insect is thus moulded out of a mass of cells, it is evident that the separate parts which then appear, such as the arches of the segments, the extremities and the oval apparatus, consist at first of only rows of simple cells. This point is here beautifully prominent, and nowhere have I observed finer illustrations of the cell-constitution of developing forms.

The development thus proceeding, each part of the dermo-skeleton becomes more and more distinct, and the increase of size of the whole is attained by the constant development of new cells. During this time, the yellow vitellus-looking mass, situated at one of the poles of the embryo, has not changed its place; it has increased somewhat in size, but otherwise appears the same. When the development has proceeded somewhat farther, and the embryo is pretty well formed, the arches of the segments, which have hitherto remained gapingly open, appear to close together on the back, thereby enclosing this vitellus-looking mass within the abdominal cavity.

It is this same vitelloid mass thus enclosed that furnishes the nutritive material for the development of new germs which would be those of the fourth brood or D; this development of germs here commences with the closing up of the abdominal cavity, and the same processes which we have just described are again repeated.

The details of the development subsequent to this point, are like those of the development of ordinary insects or of the Articulata in general; and although this ovoid germ has at no time the structural peculiarities of a true ovum—such as a real vitellus, a germinative vesicle and germinative dot; yet, if we allow a little latitude in our comparison and regard the vitellus-looking mass as the *mucous*, and the germ-mass proper as the *serous* fold of the germinating tissue, as in true eggs—if, I repeat, we can admit this comparison of parts, then the analogy of development between these germs and true eggs of insects, may be traced in considerable detail.

This comparison I have been inclined to admit at least in part, from the striking resemblance of these developing forms at certain stages, with the embryological forms of spiders as they have been figured by Herold* and as I have myself traced them.

* *Herold*, De Generatione Aranearum in ovo. Marbourg, 1824.

When, in spiders, the serous fold of the germinating tissue has extended so as to cover two-thirds of the developing form, leaving the vitelline mass on the dorsal surface near one of the poles, the whole embryo quite resembles that of a developing Aphis just before the arches of the segments close up on the back.

With this view of the relative parts of the germ, the following would be the details of the development of the different systems, and in the noticing of which I shall follow Kölliker.*

1. The germinating tissue consists of two parts; a serous and mucous fold.

2. The abdominal plates arise from the serous fold, sprout out towards the vitelloid mass, pass over it and unite on the dorsal surface of the future animal; on the opposite side are formed plates which do not unite, but are formed into the hind legs.

3. The wings are the lateral limbs.

4. The first traces of the abdominal column appear in the chain of abdominal muscles, situated between the nerves and the intestinal canal.

5. The nervous system in all its parts arises from the serous fold, as well also as the organs of sense.

6. The mucous fold or the vitellus-looking mass, serves no purpose in the formation until the closing in of the visceral plates.

7. Thus enclosed in the abdominal cavity, it is not transformed directly into the intestinal canal, but simply furnishes the material from which the component cells of said canal and its hepatic deverticula are formed. It also furnishes the material from which the new germs are formed, as already shown.

8. The heart is formed on the dorsal aspect between the mucous and serous folds. In this way the details of development closely correspond with those of the embryology of the other Articulata which I have studied; and the subject is all the more interesting as the germ-masses, from which such development occurs, in no way and at no time structurally resemble true eggs.

When the embryo is ready to burst from its developing capsule and make its escape from the abdomen of its parent, it is about $\frac{1}{16}$th of an inch in length, or more than eight times the size of the germ at the time when the first traces of development were seen. From this it is evident that, even admitting that these germ-masses are true eggs, the conditions of development are quite different from those of the truly viviparous animals; such as for instance in *Musca*, *Anthomyia*, *Sarcophaga*, *Tachina*, *Dexia*, *Miltogramma*, and others among Dipterous insects;† or in the vivipa-

* *Kölliker*, Observationes de prima Insectorum genesi adjecta articulatorum evolutionis cum Vertebratorum comparatione. Diss. Inaug. Scr. Alb. Kölliker. Turin, 1812.

A work replete with facts and interesting suggestions.

† See *Siebold* in *Frorieps* neue Notiz. iii, p. 337, and in *Wiegmann's* Arch. 1838, i, p. 197.—also his Observat. quaed. Entom., &c., p. 18.

rous reptiles,—for in all these cases of ordinary viviparity, the egg is simply hatched in the body instead of out of it. The egg moreover, is formed exactly in the same way as though it was to be deposited, and its vitellus contains all the nutritive material required for the development of the egg until the coming forth of the new individual. The abdomen of the mother serves only as a proper indus or incubatory pouch for its full development. This is true of all the the ovo-viviparous animals whatsoever.* With the viviparous Aphides, on the contrary, the developing germ derives its nutritive material from the fatty liquid in which it is bathed, and which fills the abdomen of the parent.† The conditions of development here therefore are more like those in Mammalia, and the whole animal may, in one sense, be regarded as an individualized uterus filled with germs, for the digestive canal, with its appendages, seems to serve only as a kind of laboratory for the conversion of the succulent fluids which the animal extracts from the tree on which it lives, into this fatty liquid from which the increase and development of the germs take place.

When the young animal has reached its full development as an embryo, it bursts from its encasement and appears to escape from the abdomen of its parent through a small opening *(Porus genitalis)* situated just above the anus. In the species under consideration it generally remains clinging on the back of the parent until its external parts are dry and it is able to begin life for itself. Each parent here produces from eight to twelve individuals, and if this rapid increase is continued undisturbed, through seven to nine broods, we cannot wonder at the countless numbers which appear from so few original individuals.‡

Such are the details of the embryological development of the so-called viviparous Aphides, as far as I have enjoyed opportunities for their study. We will now refer for a moment to the special points which have here been made out. In the first place, it is evident that *the germs which develop these forms are not true eggs.* They have none of the structural characteristics of eggs, such as a vitellus, a germinative vesicle and dot; on the other

* It is true that in the Scorpionidae, the eggs are developed in the ovary, but there is no reason to suppose that the conditions are here different from those of the viviparous Diptera.

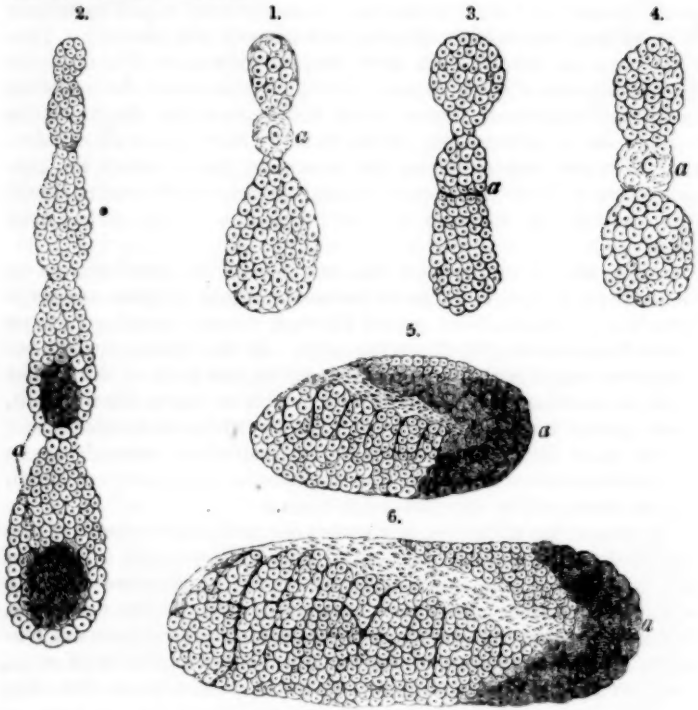
In *Oribates*, also, the eggs are developed in a kind of uterus situated directly above the ovipositor, but this appears to be only an incubatory pouch.

† This fatty matter forms beautiful crystals of margarine, and the crystallization may well be seen to take place. The forms exactly resemble those given by *Robin* and *Verdeil*, Traité de Chim. Anat et Physiol. Paris, 1853. Pl. xxxviii, fig. 2 *h*.

‡ *Réaumur* has shown that in these animals the rate of increase is so great that in five generations or broods only one Aphis may be the progenitor of five billion, nine hundred and four million, nine hundred thousand (5,904,900,000) descendants; we may well ask what would be the number of descendants where the broods were extended to eleven!!

See *Kirby* and *Spence*, Introduction to Entomology, i, p. 175.

hand, they are, at first, simple collections, in oval masses, of nucleated cells. Then again, they receive no special fecundating power from the male, as is the necessary preliminary condition of all true eggs; and, furthermore, the appearance of the new individual is not preceded by the phenomena of segmentation, as also is the case with all true eggs. Therefore their primitive formation, their development, and the preparatory changes they undergo for the evolution of the new individual, are all different from those of real ova.*



EXPLANATION OF FIGURES.

Figs. 1, 2, 3 and 4, represent the egg-like buds in different stages of development—and as they are successively formed sprouting off from the internal surface of the parent.

Fig. 1, *a*, shows a bud consisting of a single cell and situated between two larger. The same is true of

Fig. 4, *a*, where, however, new cells are about to be developed around the central one.

Fig. 3, *a*, shows the formation of a new bud by a constriction process.

Fig. 2, *a*, shows first appearance of the vitellus-looking mass near one extremity of bud.

Fig. 5, shows the appearances of the marking out of the embryo from the germ-mass.

Fig. 6. Further development and different parts appearing. *a*, represents vitelloid mass, in both figures.

* *Milne Edwards* thinks he has found true *ova* and ovaries in the viviparous forms of these animals. (Quoted by *Dr. Carpenter* in Brit. and For. Med. Chir. Rev., 1849, iv, p. 443.) I think he must have been deceived, as I was at first, by the general appearances which, unless carefully examined, closely resemble those of true oviparous individuals.

Another point is, *these viviparous individuals have no proper ovaries and oviducts.* Distinct organs of this kind I have never been able to make out. The germs are situated in moniliform rows, like the successive joints of confervoid plants, and are not enclosed in a special tube. These rows of germs commence, each, by a single germ-mass which sprouts from the inner surface of the animal, and which increases in length and in the number of its component parts from the successive formation of new germs by a constriction-process as already mentioned. Moreover, these rows of germs which, at one period, closely resemble in general form the ovaries of some true insects, are not continuous with any uterine or other female organ, and therefore do not at all communicate directly with the external world. On the other hand, they are simply attached to the inner surface of the animal, and their component germs are detached into the abdominal cavity as fast as they are developed, and then escape outwards through a *Porus genitalis*, exactly as is the case with the eggs of fishes.* Here, then, comes the important question, What interpretation shall be put upon these reproductive parts—these moniliform rows of germs? Ignoring all existing special theories relating to reproduction, the observing physiologist would be left no alternative but to regard them as *buds*, true gemmæ, which sprout from the inner surface of the Aphis, exactly like the buds from the external skin of a Polyp.†

Before proceeding to a discussion of the relations of this important conclusion to which we have just arrived, it may be well to refer to the views of others upon the exact signification of these singular reproductive phenomena.

Those old entomologists, such as Bonnet, Réaumur, Degeer, &c., who were the first to observe, besides verifying beyond all doubt, these peculiar phenomena, all believed that each brood constitutes a separate generation, and that the reproduction takes place by true ova, as in the common generative act of other insects. This wide deviation from the ordinary course of nature as it seemed to them, they attempted to explain and reconcile by various theories. Thus, Réaumur‡ affirmed that these viviparous individuals were androgynous; and, in later times, Leon Dufour,§ who knew too well the anatomical structures of insects to believe with Réaumur that they could be hermaphrodites, referred these phenomena to spontaneous or equivocal generation.

* These observations of mine on the special anatomy of the reproductive parts of viviparous Aphides, agree with those of *Siebold*, who studied the subject with much care several years since. See *Froriep's* neue Not. xii, p. 308. *Siebold*, however, regarded them as true ovaries and oviducts, but without any of the usual appendages which are found in the true oviparous Aphides.

† I would insist upon this wide and important distinction between buds and ova. The structure and conditions of all ova are the same, and there is no passage between them and buds. But this point will be enlarged upon hereafter.

‡ *Réaumur*, loc. cit. Mém's.

§ *Léon Dufour*, Recherches Anat. et Physiolog. sur les Hémiptères, Paris, 1833.

Morrem,* who made somewhat extended researches on the anatomy of *Aphis persicæ*, and especially of its generative organs, advanced the novel theory, that these broods were developed in the body of the virgin parent, by a previously organized tissue becoming individualized and assuming an independent life, exactly, as he believed, to be the case with Entozoa. To each and all of these views, it scarcely need be said that they would be wholly inadmissible according to the present established doctrines of physiological science, even had we no directly controverting observations.

But, there are other explanations or views which deserve more attention. The first of these is that advanced by Kirby and Spence.† According to them "One conjunction of the sexes suffices for the impregnation of all the females that in a succession of generations spring from that union." In support of the reasonableness of this hypothesis, they quote several instances which they regard as of analogous character; thus, they say in regard to the hive-bee, that "a single intercourse with the male fertilizes all the eggs that are laid for the space of two years."

In this connection should be mentioned the similar hypothesis advanced for a like purpose by Jourdan.‡ According to him many Lepidoptera lay fertile eggs when completely isolated from the males: such are, *Euprepsia casta*, *Episema cæruleocephala*, *Gastropacha potatoria*, *G. quercifolia*, and *G. pini*, *Sphinx ligustri*, *Smerinthus populi* and *Bombyx querci*.

But, all these cases have really no strict analogy with that of the Aphides in question; for there is not, as with these last, a succession of innately fertile individuals, but only females which are capable of producing several broods from a single coitus, or after having been long removed from the males which may even then be dead.§ Late researches upon the minute anatomy of the generative organs of insects have furnished results by which these phenomena, seemingly strange at first, can be explained. All these insects which are thus capable of laying fecundated eggs again and again after the first impregnation, have a *Receptaculum*

* *Morrem*, Anat. de l'Aphis persicæ, in the Ann. d. Sc. Nat., v. 1836, p. 90.

† *Kirby* and *Spence*, Introduction to Entomology, iv. p. 161.

‡ *Jourdan*, Manuel de Physiologie, par J. Müller, Trad. de l'Allemand. etc., par A. J. L. Jourdan. Deux. éd. rev. et annots., par E. Littré, ii, p. 599, note.

§ *Siebold* has made observations upon allied phenomena occurring in the Psychidae, which are of no little interest. He has shown that in the genera *Psyche* and *Fumea*, the alleged reproduction, *sine busina*, is unfounded—these insects having well formed internal genital organs, and the male being adapted to impregnate the female while the latter is in her case. But in the genus *Talaporia*, *Siebold* has shown that there is propagation *sine concubitu*, exactly as occurs with the Aphides. See Ueber die Fortpflanzung der Psyche: Ein Beitrag zur Naturgeschichte der Schmetterlinge, in *Siebold* & *Kölliker's* Zeitsch. i, 1849. p. 93; but, for his last researches on *Talaporia*, see his Bericht üb. d. entomol. Arbeiten d. schles. Gesellsch im J. 1850—or its English transl. in the Trans. of the Ent. Soc. N. S., i, p. 234.

seminis connecting with the oviduct, in which the semen is deposited during coition and where it may be preserved without losing its vitalizing power, for several months.* Thus, by this provision, the males, having copulated with the females in the autumn, may immediately die, while these last, hibernating, produce in the spring, fertile ova; and in the instance of the *Bombus americana*, such a coition suffices for all the three broods which are produced the ensuing summer.

Another explanation of these curious phenomena, and which has attracted some attention, as well from its singularity as from the eminence of its propounder, is that of Owen, advanced in his Hunterian Lectures in 1843.†

He affirms that the larval Aphides are productive in virtue of the successive continuation from brood to brood of a portion of the primitively fertilized germ, and which material product or leaven is not exhausted until nine to eleven generations. I will quote his own language: "In the Aphides the corresponding vitelline cells retain their share of the fecundating principle (which was diffused through the parent egg by the alternating, fissiparous, liquefactive, and assimilative processes) in so potent a degree, that a certain growth and nutritive vigor in the insect, suffice to set on foot in the ovarian, nucleated cells, a repetition of the fissiparous and assimilative process, by which they transform themselves in their turn into productive insects; and the fecundating force is not exhausted by such successive subdivision until a 7th, 9th, or 11th generation." This same doctrine, the successive inheritance of a portion of the primary germ-mass from brood to brood, and by means of which the fertile germs are continued, this doctrine, I say, is repeated in full in this author's work on Parthenogenesis, and I will here quote one sentence, not only in illustration of this, but to show how different his own observations on the development of these animals, are from mine, just described. He says, "One sees such portion of the germ-mass taken into the semi-transparent body of the embryo Aphis, like the remnant of the yolk in the chick. I at first thought it was about to be enclosed in the alimentary canal but it was not so. As the embryo grows, it assumes the portion of the ovarium, and

* For many details on this subject of the *Receptaculum seminis*, see *Siebold, Müller's* Arch., 1837, p. 392; also in *Wiegmann's* Arch., 1839, i, p. 107, (*Vespa*), and in *Germars* Zeitsch., ii. 1840. p. 442, (*Culex*). See also *Stein*, Vergleich. Anat. &c. 1847. p. 96, 112.

I cannot but believe that the anomalous reproductive conditions of the Cynipidæ will, at last, have a solution equally as satisfactory. See *Hartig, Germars* Zeitsch., ii, p. 178, and iv, p. 395. See also *Siebold* and *Stannius* Comparative Anatomy, transl., i, § 348, notes 1 and 4.

† Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals, &c. London, 1843, p. 233. This explanation is lately insisted upon (strange to relate) in his recent work "On Parthenogenesis, or the successive production of procreating individuals from a single Ovum." London, 1849.

becomes divided into oval masses and enclosed by the filamentary extremities of the eight oviducts. Individual development is checked and arrested at the apterous larval condition. It is plain, therefore, that the essential condition of the development of another embryo in this larva is the retention of part of the progeny of the primary impregnated germ-cell."—p. 70.

This view of Owen, so ingeniously advanced, and which he has made subservient for the chief support of his new doctrine of Parthenogensis, is indeed plausible and seems at first satisfactory: but, as I hope to show, it will not bear analysis.

In the first place, it is evident that Owen does not recognize any physiological difference between a *bud* and an *ovum;* this is clear from what he remarks in the first quotation, but in his work on Parthenogenesis he has said so in as many words. "The growth by cell-multiplication producing a bud, instead of being altogether distinct from the growth by cell-multiplication in an egg, is essentially the same kind of growth or developmental process.—p. 45.

Here is a fundamental error which, if not removed, will obscure all our views of the physiology of reproduction. I have already insisted upon the necessity of this broad distinction between these two forms, a necessity based not only upon differences of anatomical constitution, but also upon physiological signification. An *Ovum* is the exclusive product of an individual of the female sex, and is always formed in a special organ called the ovary. It is the particular potential representative of the female, and has its ulterior development only from its conjunction with a corresponding element of the opposite or male sex; and zoology presents no instance where there is development from eggs, unless these conditions of the two sexes are fully carried out.

A *Bud*, on the other hand, is simply an offshoot from the form on which it rests, a portion of the animal capable of individual development. It sustains, therefore, no relations to sex, and, in truth, is widely separated in its ulterior signification from that cycle of processes conceived in a true oviparous reproduction.

All physiologists who have carefully studied embryological and developmental processes, must feel the correctness and importance of this distinction which lies in realities and not in words.

It is true that a bud and an ovum are composed each of the same elements,—simple nucleated cells; but in one, these cells are simply in a mass, while in the other, they have, throughout the animal kingdom, high or low, a definite and invariable arrangement. Then again as to the constitution of each and both being, on the whole, of nucleated cells, it may be said, that it could hardly be conceived to be otherwise, for nucleated cells are the elementary components of all functional organized forms; and it may be added, moreover, that he knows little of the high-

est physiology who has not learned that widely different teleological significations may be concealed beneath isomorphic animal forms.

I have thus dwelt rather lengthily upon this point because I think it is a vital one in our subject, and the possession of clear ideas thereon will be found singularly conducive to our correct appreciation of this whole class of anomalous phenomena under discussion. But we will revert to the subject of Owen's hypothesis.

As to the chief point in this hypothesis, the continuation of the primary germ-mass as a leaven, from brood to brood, it requires but little thought to perceive that it is physically impossible. I would first allude to Owen's statement, quoted above, that a portion of the germ-mass is taken into the abdomen of the embryo Aphis, and as he thinks, assumes, without any change, the position of the ovarium. By this he refers, undoubtedly, to the vitellus-looking mass I have described in my observations, and according to which, also, it appeared to serve only as the nutritive material out of which the digestive organs and the germs are formed. Moreover, I feel quite sure that the germ-cells are new cells formed in the abdomen, and not those derived from the parent.

But the point I wish to enforce, is, that even admitting that individuals B may contain an *actual residue* of individuals A, it is clearly evident that this succession must stop with brood B; for these residual germ-cells which compose B in its earliest condition are lost in the developmental processes, and the germs of individuals C, which are found in B, are, each, primarily, nucleated cells formed *de novo*, as I have observed and above described. With these observed conditions of development, it is impossible for the individuals of the successive broods to inherit the original spermatic force in the continuation of the original cells.

The hypothesis of Owen, therefore, plausible and ingenious as it may seem, does not appear to me to accord either with observed facts, or with the soundest physiology of the reproductive processes. I may here remark also, that his doctrine of Parthenogensis, based as it is upon the conditions of the hypothesis in question, cannot, as such, be sustained, for the same reasons, and all its phenomena would appear to find their solution either in Steenstrup's doctrine of "Alternation of Generations," so-called, or in the conditions of true gemmiparity—admitting, provisionally, that Steenstrup's doctrine, and gemmiparity, include really different physiological conditions.

But the most important explanation advanced, and the last which I shall notice, is that offered by Steenstrup* in his doctrine

* On the Alternation of Generations, or the Propagation and development of Animals through Alternate Generations, a peculiar form of fostering the young in the lower classes of Animals. Transl. by the Ray Society, London. 1845—passim.

of the "Alternation of Generations," and of which it forms a chief support. The details of this peculiar doctrine of Steenstrup I need not here furnish; they are well known to all physiological anatomists. Its features, however, may be expressed in a formula-like manner. Individuals A, produce true fecundated eggs, from which are hatched individuals B, which are unlike their parents in all zoological respects, but in which are developed spontaneously and without any reference to sex, germs which ultimately become individuals like A, and so the cycle of development is completed. These intermediate individuals, B, Steenstrup has termed nurses (*Amnen*), and he regards them as distinct animals subservient for a special end; he therefore considers that B constitutes a real generation.

Instances of such phenomena are found in the lower orders of the animal kingdom—Polyps, Acalephs and Worms; and late research has shown that they are more or less common throughout the whole of the Invertebrata.

The difference between alternation of generation and metamorphosis is too marked to require illustration; in the latter there is the same individual throughout, and the developmental processes, although concealed beneath different exteriors, are regular and normal; with the former, however, this chain of development is broken by one form being developed in another, this intermediate form serving as a stepping-stone for a higher and ulterior development. Another important point in this alternate reproduction, is, that in each new change some real progress is made—the nursing-form being manifestly inferior to the individual to which it gives rise.

Steenstrup regards the Aphides as furnishing the most perfect examples known of nursing individuals, and, on the whole, as constituting typical illustrations of this doctrine he has advanced.*

But if this doctrine implies conditions other than those which belong to true gemmiparity, it does not appear to me that it has any support in the phenomena in question of the Aphides. And although I am inclined to believe, as I shall soon show, that all these phenomena, essentially, may be of the same nature, yet there can be no doubt that the manifestations are here somewhat peculiar. With the Aphides there is no real morphological progress made in each brood, for the viviparous individuals are, zoologically, as perfect in every way, as those which are oviparous, except in their want of true sexual generative organs. I have shown that, in the one species here described, they had well developed wings like the true sexual individuals. Moreover, each brood, from the first to the last, inclusive, is merely a repetition of the same. But these conditions are external and economical, and, instead of offering these prominent examples as evidence against the validi-

* See, *Steenstrup*, loc. cit., p, 112.

ty of Steenstrup's doctrine, I would rather present them as broadly indicating that, after all, this doctrine in question involves no conditions excepting those belonging to a modified form of gemmiparity. All the instances of Polyps, Acalephs, Worms, Insects, &c., all would then be classed in the same category, and the variations in manifestation would belong rather to the economical relations of the animal, than to any intrinsic difference of physiological process. Thus the Distoma-nurses instead of being developed to a condition resembling at all their parent, remain persistent on a low form, and not only is their whole zoological character undeveloped, but they also experience morphological changes from the developmental process which immediately go on within them. All this is in perfect keeping with their economy, as animals, for the low order of their conditions of life does not necessitate a higher and more truly zoological form of these nurses from which are to be developed the true animals; were it otherwise, I cannot but believe that both the nurses and the grand-nurse of Distoma, would quite resemble the original animals. In the case of the Aphides, the economical conditions are different, and finely illustrate this point.

The Aphis-nurse, in virtue of its very typical structure as an insect, must live under higher conditions, and so its development, zoologically, proceeds to a corresponding point; this point is where it, as an insect and as an Aphis, can furnish the nutritive material for the development of its endogenous germs.

Herein, then, would appear to consist the prominent morphological differences observed in this category of phenomena, and I need not labor further to show that they are irrelevant of the primary essential conditions of these curious processes.

Such appears to me to be the highest, both physiological and zoological, interpretation that can be advanced for these phenomena which Steenstrup has so ingeniously collected and collated; and to advance the view that these intermediate individuals or nurses, are not intrinsically and zoologically the same as their parents, but furnish examples of how dissimilar animals may arise from a common stock—to put forth this view, I say, is to advocate a doctrine in physiology, as mischievous as it is deeply erroneous. I think, therefore, that the doctrine of Steenstrup may prove to be unfounded as far as it would involve, intrinsically, new phenomena in the processes of reproduction; and, as I have said on a preceding page, all its conditions may find their illustration and solution in the various phases of gemmiparity.*

* This statement is made perhaps more strongly and exclusively than the present state of our knowledge would warrant, but I throw it out much in a suggestive way. There is no subject in Physiology more interesting and comprehensive than that of *Gemmation*; the important question now is, does it, as an individual process, embrace all the categories of phenomena treated by *Lovén*, *Steenstrup*, &c., these phenomena varying extrinsically, according to economical conditions, or do they (the phenomena) imply something beyond and dissimilar from gemmation?

If in this discussion of some of the highest relations of physiology, we have not wandered too far from our subject proper which we have thereby sought to illustrate indirectly, we will revert to the thread of its discourse for a few concluding remarks.

The final question now is, what is the legitimate interpretation to be put upon the reproductive phenomena of the Aphides we have described? My answer to this has been anticipated in the foregoing remarks. I regard the whole as constituting only a rather anomalous form of gemmiparity. As already shown, the viviparous Aphides are sexless; they are not females, for they have no proper female organs, no ovaries and oviducts. These viviparous individuals, therefore, are simply gemmiparous, and the budding is here internal instead of external as in the Polyps and Acalephs; it, moreover, takes on some of the morphological peculiarities of oviparity, but all these dissimilar conditions are economical and extrinsic, and do not touch the intrinsic nature of the processes concerned therein.

Viewed in this way, the different broods of Aphides cannot be said to constitute as many true generations any more than the different branches of a tree can be said to constitute as many trees; on the other hand, the whole suite from the first to the last constitute but a single true generation. I would insist upon this point as illustrative of the distinction to be drawn between *sexual* and *gemmiparous* reproduction. Morphologically, they have, it is true, many points of close resemblance; but there is a grand physiological difference, the true perception of which is deeply connected with our highest appreciation of individual animal life.* A true generation must be regarded as resulting only from the conjugation of two opposite sexes—from a sexual process in which the potential representations of two individuals are united for the elimination of one germ. This germ-power may be extended by gemmation or by fission, but it can be formed only by the act of generation, and its play of extension and prolongation by *budding*, or by division, must always be within a certain cycle, and this cycle is recommenced by the new act of the conjugation again of the sexes.

In this way, the dignity of the ovum as the primordium of all true individuality is maintained; and the axiom of Harvey, *omne vivum ex ovo*, stands as golden in physiology. The buds may put on the dress and the forms of the ovum, but these resemblances are extrinsic and in fact only an inheritance from their great predecessor.

* In this view as well as in several others herein discussed, I am pleased to say that I have the support of so learned a physiologist as Dr. Carpenter. See his Review "On the development and Metamorphoses of Zoophytes" in the Brit. and Foreign Med. Chir. Rev., 1848, i. p. 183; and "On Reproduction and Repair" in Ibid. 1849, ii. p. 419.

These phenomena thus interpretated, furnish us an excellent key to many others which have long been regarded as anomalous, in the history of development.

I refer here to the so-called hibernating eggs (*Wintereier*) which are found in many Invertebrates. These I have not seen, but they have been carefully described by several very trustworthy observers. These so-called eggs consist of oval masses or cells invested with a capsule, but in which no germinative vesicle and dot have ever been seen. Structurally, therefore, they do not resemble eggs, and it is from their form and ulterior development only that they have received this name. Moreover, they sustain none of the usual relations of eggs to the sexual organs, and, as far as I am aware, no one has witnessed their development in the ovaries. These bodies have been observed in *Hydatina** and *Notommata*† among the Infusoria; in *Lacinularia*‡ among the Rotatoria; and in *Daphnia*§ among the Crustacea. In all these instances they hatch without the aid of the male, the existence of which sex was once doubted from its infrequent appearance.

Now I regard these hibernating eggs as merely egg-like *buds* exactly corresponding to the germs of the viviparous Aphides. In other words, there are in the animals I have just mentioned, certain individuals which reproduce by buds which are developed under rather anomalous conditions; and I will add in conclusion, that I suspect that this gemmiparous mode of reproduction will be found to be far from uncommon among most of the Invertebrata, when our researches into the history of their development shall have been more widely extended.‖

* *Ehrenberg*, "Die Infusionsthierchen," p. 413.

† *Dalrymple*, Philos. Trans. 1849. p. 340.

‡ *Huxley*, Quarterly Jour. Micr. Sc. 1852. i, p. 13.

§ *Müller*, Entomostraca, p. 84. Tab. xi, fig. 9–11, Tab. xii, fig. 5. Also, *Ramdohr*, Beiträge zur Naturgesch. einiger deutschen Monokulus-Arten, 1805. p. 28; *Strauss*, Mém. sur les Daphnia, in the Mém. du Mus. d'Hist. Nat., v, p. 413. Pl. xxix; *Jurine*, Histoire des Monocles, 1820. p. 120. Pl. xi, fig. 1–4. Jurine calls these aggregated eggs "La maladie de la selle."

There is, moreover, reason to believe that these anomalous reproductive conditions occur in nearly all the Entomostraca:—see *Siebold* and *Stannius's* Comparative Anat. My transl. vol. i, my note under § 292, note 4.

‖ Notice may here be given of some curious observations, which *Filipi* (Ann. Nat. Hist., ix, 1852, p. 461) has furnished on the development of the Pteromalidæ. A *Pteromalus* lives in the ova of *Rhynchites betuleti*; in each of these ova, there is seen, soon after its deposit, a minute infusorial animal, with a tail by which it moves briskly about among the vitelline cells. It soon ceases to move, however, and in its interior appears a vesicle which increases and changes into a larva which is that of *Pteromalus*; this larva becomes a pupa, and, after eight or ten days, changes to the perfect insect which escapes from the ovum.

If these observations are verified, we have here a case exactly like that of the Aphides, excepting that like the *Distoma*, the intermediate budding form is very low, and takes on none of the zoological peculiarities of the parent. But these statements need corroboration, for they do not agree with the history of other species of *Pteromalus* whose development is well known. See also, the wonderful gemmiparous phenomena related by Siebold of *Gyrodactylus*; *Siebold* and *Kolliker's* Zeitsch. f. wiss. Zool., i, 1849. p. 347.

P. S.—I regret that I should not have seen until now, when this paper is concluded, the important writings of Leydig on the subject under discussion. In his article "Einige Bemerkungen über die Entwickelung der Blattläuse" in *Siebold* and *Kolliker's* Zeitsch. f. wiss. Zool. ii, 1850. p. 62, he speaks of his former observations in the Isis, 1848, iii, p. 184. These I have not seen, neither also a work to which he refers, of J. Victor Carus, (Zu näheren Kenntniss des Generationswecshels, Leipzig, 1849.) Leydig in his criticism of Carus's views, expresses the opinion that the development of the viviparous Aphides is, histologically, like that of the Articulata in general. According to him, also, the germ-bodies undergo processes corresponding to those of impregnated eggs. These statements of Leydig, who is an excellent observer, have induced me recently to repeat my observations; but this afforded the same results as before, viz.: that the germ-bodies out of which are developed the viviparous Aphides, have no true histological identity with eggs.

ART. VIII.—*Mineralogical Contributions;* by JAMES D. DANA.

1. *Brooke and Miller's Mineralogy.*

THE reviewer of this work in volume xv, page 41, mentions but briefly its importance in a crystallographic point of view. It is in this department eminently an original work, the result of special researches on the crystallization of very many of the species, with the measurement and calculation of their angles. It must therefore be long a standard work. Mr. Brooke has been indefatigable in his crystallographic studies, and early became prominent in English mineralogy by his Introduction to Crystallography, published in 1823; Professor Miller is no less distinguished for the precision and accuracy of his labors in physical science. The number of angles of each species given is very large, and there is a mathematical exactness and system in the selection of those which are mentioned, according with the methods of calculation. The circles filled in with dots, accompanying the descriptions of the species, afford much assistance in apprehending the positions of planes. The eye when looking down upon a crystal from above, an axis being in the line of vision, sees the planes arranged symmetrically, around the axis: and on describing a circle about the axis as its centre, if dots corresponding each in position to the normal of a plane, be placed in the circle, an ideal view, or rather the mathematical essence of the crystal, will be presented. Such are the circles referred to. The great deficiency in the work is the want of proper figures to aid in applying these ideal representations so as to give an actual shape to the mind correspond-

ing with the habits of the species. Moreover, another method of tabulating the planes may accomplish the same result more simply, we think, and one in which the planes shall be indicated by their true expressions, instead of arbitrary letters. The method alluded to, has already been presented in this Journal, in the writer's paper on Sphene and Euclase. It contrasts with the method in Brooke and Miller's work mainly as Mercator's projection of a sphere contrasts with the spherical projection.

The method of calculation adopted, is based for the most part on spherical trigonometry. It is less general in its formulas and less elegant, we think, than the system from analytical geometry, but sometimes affords more concise equations. The hexagonal system is rather at variance with itself in the method used. The planes are referred to *three* axes,—the three lines that connect the centres of opposite faces in a rhombohedron. The plan is not objectionable as regards the rhombohedral section of the hexagonal system. But in the hexagonal section of this system, in which the symmetrical parts are by sixes instead of threes, the planes of a simple dihexagonal pyramid require two sets of symbols in place of one: that is, where two identical planes occur about the same angle of a hexagonal prism, as in beryl, these must have different mathematical expressions. This leads the mathematician to no error; but tends to perplex a simple subject for the student.

But these are minor points, and leave the work still, the most accurate, thorough, and original work on the crystallization of minerals that has been published.

2. *Von Kobell's "Mineral-Namen."**

Prof. von Kobell, introduces the subject of his work by many judicious and pithy remarks on the multitude of synonyms in mineralogy, and the varieties of structure, derivation and formation, exhibited among the names of species. He next classifies the names according to their derivation, and explains their etymology. He presents, *First*, a catalogue of those names that are of mythological origin. *Second*, leaving the gods and mythology, he passes to those derived from the names of cultivators of the science, collectors of specimens, patrons, statesmen, and what not, among whom, about two hundred and thirty are hereby commemorated,—not immortalized, for amid the fatality to which mineral species are subject and the custom of change in authors, very many of the names are already in the rubbish heaps of the science. *Third*, comes a list of the names derived from localities; *fourth*, those alluding to the structure of the species; *fifth*, those based

* Die Mineral-Namen und die mineralogische Nomenklatur, von Franz von Kobell. 162 pp. 8vo; München, 1853. Briefly mentioned in this Journal, page 304, vol. xvi.

on color; *sixth*, on other physical characters; *seventh*, on chemical composition and reactions; *eighth*, names derived from other peculiarities, uses, arbitrary or fanciful allusions; *ninth*, those of unknown or doubtful origin. After speaking of the importance of a universal nomenclature for different countries, Prof. von Kobell lays down several rules for nomenclature in mineralogy, requiring that names derived from names of persons or localities should be written according to their original orthography, and not altered for each different language; that the Greek language should be used, rarely the Latin, for the derivation of other names; that the earliest name of a species should be retained, only when correctly formed in accordance with these principles: and he gives a list of some names that have been more or less recently proposed as substitutes for earlier objectionable names, the general adoption of which he observes would tend to make mineralogical language the same the world over. The greater part of these names are already accepted in the science.

It may be doubted whether, by carrying out with full strictness, his laws, we may not in some cases, create more confusion than we avoid, especially in the case of species well known in the arts. For example in substituting, as is proposed, *Liparite* (Glocker) for *Fluor* or *Fluor spar* (Fluss or Flusspath, of the Germans), we are giving a new word to science, without special significance in itself, and making confusion between the terms of the arts and science. As mineralogy is but a semi-science, and its nomenclature but a convenient means of designation without a proper scientific basis, we should hesitate before adopting new names in cases like the above. Print it *Liparite* and still the mineral will be called *fluor*.

Blende or *Zincblende* is another case of this kind. We cannot consider Glocker's *Sphalerite* a needed substitute for the old name. *Hematite* is an unfortunate substitute for specular iron, as it is restricting to narrow limits an old name of wider signification; and in this country, it is the most common designation of the species limonite. It is however coming into common use in Europe and Great Britain. *Arsenite* for arsenous acid, and *Chromite* for chromic iron, are objectionable names, as the termination is a chemical one for a section of salts. *Galenite* is not an improvement on Galena, as the word is as appropriate without the "*ite*."

Horn silver (Hornsilber of the German), although two words and obnoxious to the criticism of not being Greek, is significant and contains fewer syllables and letters than Kerargyrite,—or Cerargyrite as the word should be written with us. For *Silver glance*, early called *Argyrose* by Beudant, Haidinger's name *Argentite* is adopted by von Kobell. *Clay* is dignified with the name of *Argillite*. *Azurite* of Beudant (*Blue Malachite, Kup-*

ferlasur, of the Germans, *Chessylite* of Brooke and Miller) is written Lasurite, contrary to a canon laid down by the author, requiring a Greek etymology; while *mispickel* is thrown aside for *arsenopyrite* of Glocker, as it wants this honorable origin. —The mineral called *Fahlerz* by the Germans, has given great trouble to English mineralogists, partly on the ground that a name like Gray Copper, consisting of two words, is objectionable, and partly because of the desirableness of a name common to both countries: sometimes the German name has been used, although one of the least significant of names, meaning simply Gray-ore; and sometimes Fahl-ore is employed, as if preferable to the translated expression. Haidinger's name, *Tetrahedrite* is adopted for it by von Kobell to the rejection of Beudant's *Panabase*, which is long prior in date, but less appropriate and badly compounded.—The mineral named *Lölingite* by Haidinger, and so accepted by von Kobell, was named *Mohsine* by Chapman, in 1843, and *Leucopyrite* by Shepard in 1837, and this last name has, therefore, the best title.

The name given by the writer to the so-called *Common* or *Oblique mica*, on the ground that the old name was bad, is overlooked by von Kobell. The word *Muscovite* was intended as no indignity to the Czar or his subjects, and commemorates the old name Muscovy glass, as well as the mineralogical fact that Russia has long been famous for affording gigantic plates of this species. Von Kobell adopts the name *phengite* for the mineral, Breithaupt's *generic* name for the biaxial micas.—The name *Calamine* is adopted for *Electric Calamine*, and *Smithsonite* for Carbonate of Zinc, as was long ago proposed by Beudant. Brooke and Miller have unfortunately reversed these names.

Although some objections have been suggested, the names and principles of von Kobell will command special attention. A system of names once agreed upon, would in part stop off the crowd of synonyms that are constantly coming in upon the science:—only in part, however, as long as there exists more ambition to attach a name to a stone than carefully to determine and accurately describe a species. Add to Prof. von Kobell's principles, one more,—truth and not self as the end of every investigation,—and the remedy would be nearly complete.

3. *The "Krystallo-Chemische Mineralsystem of Gustav Rose."*

Prof. Rose has here presented a modification of the Berzelian system of classification of minerals, in which the modifying principle is derived principally from Crystallography. The first part of the work, after an introductory chapter, contains a view of the distribution of species, according to their composition, as mentioned in volume xv, of this journal, page 430.

The application of crystallography, which is made in the latter part of the volume, consists in arranging the species of the several subdivisions in columns, according to the system of crystallization to which the species belongs. The divisions are thus broken up into natural groups, to a considerable extent, and a very interesting exhibition of the relations of the species is afforded.

If any objection can be made to this arrangement, it is one that science is not at present able wholly to overcome. There are various indications among the results of recent researches, that *peroxyd* and *protoxyd* compounds cannot be necessarily separated, and also that *hydrous* and *anhydrous* species may belong to one and the same group,—whether we adopt the views of Scheerer or not. Even in the method of Prof. Rose some exceptions are allowed, such as the placing of spodumene near augite, (rightly as we think,) although it belongs, in fact, to a following subdivision if the constituents of the species be considered. The exceptions are prophetic of a higher and wider principle than has yet been clearly brought out.

This volume contains extended notes, and original views on many mineral species. Among them it is observed that Sillimanite and Kyanite are widely different in specific gravity, and are probably distinct species; that ryacolite is probably a bad species based on an analysis by the author (G. Rose) of a specimen of feldspar mixed with some Nepheline; that Lepolite of Orrijervi, Amphodelite of Lojo and Tunaberg, and Walmstedt's Scapolite of Tunaberg are nothing but anorthite, and Linseite, Rosite and Roselan are altered anorthite; besides many other facts and opinions of interest. These various observations through the volume, the classification, and the chemical formulas of species, by one of the ablest mineralogists of the age, render the work of great value and authority in the science.

4. *Crystallization of Haydenite of Cleveland.*

Haydenite from Jones's Falls, Maryland, was described by Levy, as monoclinic in its crystallization, and he gave for the angle of the rhombic prism 98° 22′, and for the inclination of the basal plane on the sides, 96° 5′. On account of the close resemblance of the crystals in form, to those of Chabazite, the species has long been suspected to be nothing but that species; but the difference in form and the angles, as well as in the analyses, has seemed to favor its being distinct. In some recent measurements, the writer obtained the angle 97°–98°, sustaining the ap-

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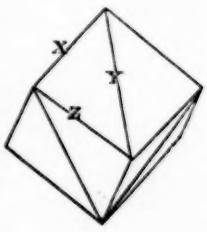
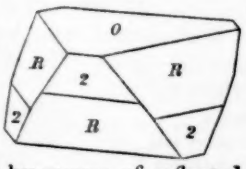


parent difference. The crystals are coated with Green earth, and only a part of the surfaces examined in this measurement were uncovered. On investigating further these and other crystals, the writer finds that the crystals are in fact scalenohedral, although nearly identical with the rhombohedron (R), the obtuse terminal edge (edge Y) differing but little from the inclined diagonal of R. Its faces are smooth and polished. While the positions of the planes show that the form is actually rhombohedral and not monoclinic, they also explain how different angles might be obtained in different directions, especially as the alternate pyramidal faces of the scalenohedron are often very unequal. This scalenohedron, since it is formed from a bevelment of the terminal edges of R, has the general sign $\frac{2}{3n-1}R^n$. The angle Y, (or that at the more obtuse or longer pyramidal edge,) by measurement is 175°–176°; X, (or that at the acuter or shorter edge,) is 97°–98°. These angles give approximately $n = \frac{1}{9}$, and lead to the sign $\frac{2}{7}R^{\frac{1}{9}}$; but calculation from this expression and the fundamental form of Chabazite makes the angle X = 100° 30′, which is too large; for Y it gives 176° 8′, and for Z 84° 46′. The measurements are however only approximative. The same scalenohedron, apparently, occurs in the Chabazite crystals of Nova Scotia, but the planes in those crystals are so made up of striæ that the angles are hardly measurable; such striæ have been considered an oscillation between the planes R and $-\frac{1}{2}R$. There can be little doubt that the species is crystallographically and physically chabazite. The analyses are adverse to this conclusion. But Prof. Silliman states that he has reason to doubt his results; and in those by Delesse, part of the iron was probably protoxyd, as obtained by Silliman. Small crystals of Heulandite (Beaumontite) cover the specimens, and are often implanted on the Chabazite. The Heulandite and green earth are probably of cotemporaneous formation, and subsequent to the Chabazite in origin. Some of the Chabazite crystals consists wholly of Green earth.

5. *Crystallization of Brucite* ($\dot{M}g\,\ddot{H}$).

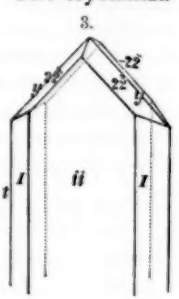
I am indebted to Mr. George J. Brush for a specimen of Brucite affording some minute crystals. They constituted a druse upon the foliated brucite in a narrow fissure in the serpentine. The crystals prove to be rhombohedral, as in the figure. The cleavage is basal, and the basal plane *O* is pearly. The other faces are brilliant vitreous in lustre. The crystals are so minute that the inclinations could be measured with the reflective goniometer only by means of reflected



light. They thus afforded the angles $O:R=119°$ (to 119° 55′) $O:2R=105°\ 30'$. These angles correspond to $R:R=82°\ 15'$, and axis $a=1{\cdot}527$.

6. *Crystallization of Hydromagnesite, and its Homœomorphism with Wollastonite.*

Messrs. Smith and Brush have recently shown that the distinctly crystallized Lancasterite is Hydromagnesite, while the pearly foliated mineral called by this name, is Brucite. The crystallizations from Texas, Pennsylvania, (Low's, and also Wood's mine,) are either massive fibrous, and radiated, looking much like Thomsonite, or flattened or acicular crystals, more or less grouped. At Hoboken the same mineral is found in very fine acicular crystals clustered together and having the aspect of natrolite. The lustre is vitreous. A distinct cleavage has not yet been observed. The Texas crystals occasionally have regular terminations, as may be seen with a magnifier of some power. They belong to the monoclinic system. With the reflective goniometer as above, I found for the angles $ii:2\breve{2}=112—113\frac{1}{2}°$; ii (back) : $-2\breve{2}=103°$; $2\breve{2}:-2\breve{2}$ (adjacent in same pyramid) $=143\frac{1}{2}$ $-145°$; opposite orthodiagonal edges of the pyramid (or y on y) over the summit $=94°$; same edge on orthodiagonal edge of prism (y on t) 133°. The last two angles were measured with Smith's goniometer attached to a compound microscope.



These measurements give for the inclination of the vertical axis, 82—83°; for the angle of the rhombic prism $i2$ (or ∞P2) corresponding to the octahedron $2\breve{2}$, 51° 26′ to 51° 32′; for the vertical axis, clinodiagonal and orthodiagonal of the octahedron $2a:2b:c=1:2{\cdot}2:1{\cdot}07$, nearly.

The prism I (or ∞P), hence would have the angle 87° 52′–88°; and taking this as the fundamental prism, then the pyramid will be formed of the planes $2\breve{2}$ and $-2\breve{2}$, (2P′2 and -2P′2), as represented in the figure. We shall also have $2i:2i$ (2P′∞ : 2P′∞) $=94°$, $O:2i$ (0P : 2P′∞) $=137°$, $ii:2i$ (∞P′∞ : 2P′∞) $=133°$.

The angle of the prism (I on I) is nearly the angle of Wollastonite, (this Jour. [2], xv, 449), and moreover $O:2i$ in Wollastonite is 137° 48′, or but 48′ different from that of hydromagnesite.

Wollastonite and hydromagnesite are hence obviously homœomorphous. To this homœomorphous group belong, as elsewhere shown,

Borax, . .	$\dot{N}a\,\bar{B}^2\,\dot{H}^{10}$	Pyroxene, ...	$\dot{R}^3\,\bar{S}i^3$
Glauber salt, .	$\dot{N}a\,\bar{S}\,\dot{H}^{10}$	Acmite, . . .	$\dot{N}a\,\bar{S}i+\dot{F}e\,\bar{S}i^2$
Hornblende, .	$\dot{R}^4\,\bar{S}i^3$	Spodumene,	$\dot{R}^3\,\bar{S}i^2+4\dot{R}\,\bar{S}i^2$, or $(\dot{R}^3,\dot{R})\,\bar{S}i^2$

and probably Laumonite, $\dot{C}a^6\,\bar{S}i^3+4\dot{A}l\,\bar{S}i^3+18\dot{H}$, which is, a Spodumene with 18$\dot{H}$.

Chapman adds Crednerite; but we have seen no measurements of the angles of that species that confirm this reference.

Hydromagnesite is formed from the alteration of Brucite, and the change is one now in progress.

7. *Homœomorphism of Gypsum and Heulandite.*

The similar pearly cleavage of Gypsum and Heulandite, two species monoclinic in crystallization, suggests the idea of an approximate isomorphism: and this is sustained by a consideration of their crystals, although not obvious in the ordinary mode of viewing their forms.

Crystals of gypsum sometimes have a hexagonal outline with a bevelment of each of the six sides, by a pair of planes, either of which pair might be taken as planes of the fundamental prism. The interfacial angles of these pairs of planes or prisms are 138° 28′, 143° 42′ and 111° 42′ (B. and M.) The first of the prisms here mentioned is also a cleavage prism, cleavage taking place, though with some difficulty, parallel to its faces. There is some reason, therefore for considering this the fundamental prism, although usually taken as planes of the fundamental octahedron. There is a second imperfect cleavage parallel to a plane truncating the edge of the prism 111° 42′ (M : M of authors), a fact that evinces the prominent importance of the plane.

If we make the former cleavage directions, the lateral faces of the fundamental prism (*I*), the latter its basal plane (*O*), the planes hitherto observed (see Brooke and Miller) will be as shown in the annexed table, in which *O* is the base, and the columns contain the different vertical zones of planes.

O					
$1\breve{i}(d)$		$1(l)$			
		$2(v)$			$2\breve{i}(m)$
$3\breve{i}(e)$	$3\breve{3}(w)$	$3(u)$		$3\breve{2}(y)$	
					$4\breve{i}(h)$
					$6\breve{i}(k)$
$i\breve{i}(t)$		$I(n)$	$i\breve{2}(x)$	$i\breve{3}(s)$	$i\breve{i}(b)$

The occurring octahedral planes are situated upon the *acute* basal edges and angles of the fundamental prism. The angle $I : I = 138°\ 28'$, $2\breve{i} : 2\breve{i}$ (clinodiagonal planes $2P' \infty : 2P' \infty = 111°\ 42'$; and the faces meeting at 143° 42′, are planes of the fundamental octahedron (P or 1). The axes,

$$a : b : c = 0{\cdot}9 : 1 : 2{\cdot}4135,$$

and the angle C (the inclination of the vertical axis) $= 66°\ 14'$.

Now in Heulandite, $I : I = 136°\ 4'$; $1 : 1 = 146°\ 56'$; and the axes,

$$a : b : c = 1{\cdot}05 : 1 : 2{\cdot}475. \quad C = 86°\ 26'.$$

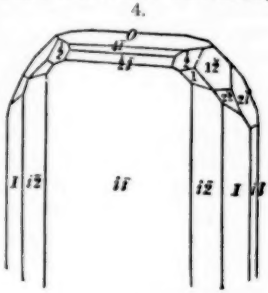
The dimensions and the angles here mentioned (excepting C) are therefore very similar in these two species, and the crystals are much alike in habit. There is a wide difference between the two in the inclination of the vertical axis which makes large discrepances in some of the angles; for example $2\breve{i} : 2\breve{i}$ in gyp-

sum is 111° 42′, in Heulandite, 98° 40′. When the axes are equal in two cases, the inclining of the vertical axis in one, diminishes the distance of its apex from the plane of the lateral axes, and consequently, the clinodiagonal prisms, and the octahedral forms on the obtuse basal edges and angles of the fundamental prism, become more and more flattened. This is the main source of the differences in angle between Gypsum and Heulandite. It shows us that in the oblique systems, we have to look to the relations of the axes, rather than to all the angles, in tracing out the resemblance of form among species.

Another inference from this case of isomorphism is that although some cases show that cleavage is a subordinate character, we may err in comparing crystals of different species, if we do not give due importance to the cleavage directions.

8. *Homœomorphism of Brookite and Columbite.*

Through the kindness of Mr. George J. Brush, I have recently received a specimen of Brookite from the Ellenville lead mine, Ulster Co., N. Y., occurring implanted on a group of quartz crystals. The mine is situated in the Shawangunk grit, a Silurian rock. The crystal has a striking resemblance in habit to that of the Columbite of Middletown, Ct., even to minor points in the arrangement of the planes. The form suggested at once the homœomorphism of the species, and this is closely sustained by the angles.



	Brookite.	Columbite.
Angle of prism $I(\infty P)$	99° 50′–100° 30′	100° 40′
$O : 2i(0P : 2P\breve{\infty})$	117° 54′	119° 40′
$a : b : c$	0·94438 : 1 : 0·84158	0·8675 : 1 : 0·8291.

The isomorphism is as close as between Celestine and Barytes.

It is of interest to compare the Columbite series (which includes Euxenite, Samarskite, Wolfram) with the Barytes series. The axes are as follows:

	Columbite.	Heavy Spar.
$a : b : c$	0·8778 : 1 : 0·8292	1·3127 : 1 : 0·81413.

Now it is apparent that a in Heavy Spar is $1\frac{1}{2}$ times a of Columbite. The relation is consequently a simple one—and so simple that the species may be considered as belonging to one and the same homœomorphous series. The plane $\breve{P}\,\infty$ of Heavy Spar and $\frac{3}{2}P\,\breve{\infty}$ of Columbite would be nearly identical in their inclinations on the base of the prism.

Such remarkable cases of homœomorphism almost show that similarity of form may exist irrespective of the elements concerned, or of any numerical ratio in the atoms of the elements

present. It is obvious that crystallization must follow, or go hand in hand with composition, but not lead the way in a classification of inorganic substances.

Hermann has endeavored to meet some of the difficulties produced by homœomorphism, by supposing two or more primary compounds as the bases of a class of species, the occurrence of which in different proportions shall form all the species of an homœomorphous group. This has an appearance of propriety as regards some groups of Silicates. But what symmetry in the constitution of the two can be made out in this way when Heavy Spar and Graphic Tellurium are compared; or Sulphur and Scorodite?—or Arragonite, Bournonite and Nitre?—or Chrysolite and Epsom salt?—or Pyroxene, Glauber salt and Hydromagnesite?—or Brookite and Columbite?—Such facts evince that homœomorphism is depedent on something beyond mere arrangement of similar atoms or of assumed parts of a species. The great principle of equality of atomic volume appears to be at the basis of it, and it would seem to matter not what the elements are; if only the resultant has a certain relation as regards atomic volume to the atomic volume of another compound, there is isomorphism.

Still there is often in related groups, a numerical relation in the elementary constitution which affords an explanation of the atomic volume relation, without looking to other considerations: and this numerical relation may be extended to the whole, when the subject is better understood. Such resemblances in homologous species, as between carbonates of protoxyds, etc., are of the most obvious kind, and have long been recognised.

The Gerhardtian view, that protoxyds and peroxyds may replace one another, taking three parts of protoxyds to one of peroxyd,—that is parts equal in oxygen—also explains in a numerical way many seeming anomalies, as in the case of the varieties of epidote, etc. Gerhardt applies it to the oxyds themselves, and seems to show that peroxyds and protoxyds crystallize alike; for in Martite, Specular iron ($\dot{F}e$) occurs in monometric forms, corresponding to the form of Periclase ($\dot{M}g$), and of Magnetite ($\dot{F}e\,\ddot{F}e$); and Laurent has recognised that the monometric and rhombohedral metals (the latter isomorphous with $\ddot{A}l$ and $\ddot{F}e$) are two corresponding series, like the two forms of a dimorphous substance.

In this way, *Axinite* and *Danburite* have a like relation and both are triclinic. In axinite the ratio between the oxygen of the bases and boracic acid, and that of the silica is 1 : 1; and the same is true for Danburite as the recent analyses of Smith and Brush show.* The formulas may be written, for axinite $(\dot{R}^3, \ddot{R}, \ddot{B},)\,\ddot{S}i$; for Danburite $(\dot{C}a^3, \ddot{B},)\,\ddot{S}i$. The propriety of reckoning the boracic acid with the bases is shown by the fact that in Tourmaline the ratio thus obtained is the only one that is constant for all

* This Journal, [2], xvi, 365.

varieties of the species, a fact admitted by Rammelsberg, but more particularly remarked upon in explanation of the isomorphism by Naumann. The formula of Tourmaline on this ground would be $(\dot{R}^3, \ddot{R}, \ddot{B})_4\, \ddot{S}i^3$.

But when we meet with such a case as that of the feldspars, where the only constant ratio is that of the oxygen of the protoxyds and peroxyds, the oxygen of the silica varying, the law for the replacement of protoxyds by peroxyds seems to have no application.

9. *Anhydrite.*

Hausmann, in his paper on the system of crystallization of Anhydrite, (Karstenite), and its homœomorphism with the Barytes series,* arrives at the following comparisons:—

		$a:b:c$	∞	$1\text{-}\breve{\infty}$	$1\text{-}\bar{\infty}$
Thenardite	$\dot{N}a\,\ddot{S}$	0·7494 : 1 : 0·5918	118° 46′	106° 18′	76° 34′ (103° 26′)
Heavy Spar	$\dot{B}a\,\ddot{S}$	0·7659 : 1 : 0·6234	116° 22′	105° 6′	78° 18′ (101° 42′)
Anhydrite	$\dot{C}a\,\ddot{S}$	0·7636 : 1 : 0·6531	115° 42′	105° 16′	81° 6′ (98° 54′)

A closer approximation of Anhydrite is obtained by making the prism m, the vertical prism $\alpha\text{-}\frac{3}{2}$, and s the brachydome $\frac{4}{3}\text{-}\breve{\infty}$. Then the axes and the above angles become,

Anhydrite, 0·73486 : 1 : 0·59398 118° 35′ 107° 22′ 77° 4′ (102° 56′).

Giving the crystal the position usually adopted for heavy spar, in which the above α, $1\text{-}\bar{\alpha}$, and $1\text{-}\breve{\alpha}$ correspond respectively to $1\text{-}\bar{\alpha}$, $1\text{-}\breve{\alpha}$ and α, the axes are $a:b:c = 1{\cdot}368 : 1 : 0{\cdot}8083$, while those of Heavy Spar in the same position are $1{\cdot}3127 : 1 : 0{\cdot}81413$. The prisms m and s in this view, are $\frac{3}{4}\text{-}\bar{\alpha}$ and $\frac{2}{3}\text{-}\bar{\alpha}$, and the octahedral planes are $m\text{-}\breve{n}$, $2m\text{-}2\breve{n}$, $3m\text{-}3\breve{n}$, with $m = \frac{3}{4}$ and $\breve{n} = \frac{2}{4}$.

The cleavage in this case is brachydiagonal and basal, with the macrodiagonal less perfect. The divergence in crystallization of the sulphate of lime from the others of the series, is a parallel fact with that of the bisilicate of lime ($\dot{C}a^3\,\ddot{S}i^2$, Wollastonite) from pyroxene.

10. *Valentinite or White Antimony and Senarmontite.*

Senarmontite is described as crystalling in the monometric system and Valentinite in the trimetric, the chemical species SbO^3 being therefore dimorphous. But it is a fact worthy of remark, that the prism $1\breve{i}$ ($\breve{P}\,\infty$) of Valentinite is identical in angle with the angle of a regular octahedron, 109° 28′. The vertical axis and macrodiagonal in Valentinite have the ratio that subsists between the axis and intermediate diagonal of an octahedron, or $\sqrt{2} : 1$, and the vertical axis (a) equals nearly the sum of the two lateral ($b+c$).

* Poggendorff's Annalen, lxxxiii, 572.

Art. IX.—*Reviews and Records in Anatomy and Physiology;* by Waldo I. Burnett.

1. *Traitè sur le venin de la Vipère*, par Fontana. Florence, 1782. p. 229.
2. *Handbuch der Entwickelungsgeschichte des Menschen*, von G. Valentin. Berlin, 1835. p. 268.
3. *Mikroskopische Untersuchungen ueber die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen*, von Thos. Schwann. Berlin, 1839. p. 165.
4. *On the Minute Structure and Movements of Voluntary Muscle.* By Wm. Bowman, in the Philos. Transact., London, 1840, pt. I, p. 457, —also, by the same, Articles, *Muscle, and Muscular Action*, in Todd's Cyclopædia of Anatomy and Physiology. 1842.
5. *On Fibre.* By Martin Barry, M.D., &c., in the Philos. Transact. London, 1842, pt. I, p. 89,—also, by the same, *Neue Untersuchungen über die schraubenförmige Beschaffenheit der Elementarfasern der Muskeln, nebst Beobachtungen über die muskulöse Natur der Flimmerhäschen*, in Müller's Arch., 1850, p. 529;—and, *On Animal and Vegetable Fibre, as originally composed of twin Spiral filaments, in which every other structure has its origin;* in the Edinb. New Phil. Jour., Oct., 1853, p. 317.
6. *Observations on the Minute Structure and mode of Contraction of Voluntary Muscular Fibre;* by W. M. Dobie, F.B.S.E., in the Annals and Mag. of Nat. Hist., iii, 1849, p. 109.
7. *Recherches sur la Formation des Muscles dans les Animaux Vertebrès, et sur la structure de la fibre musculaire en général dans les diverses classes d'animaux;* par M. le Docteur Lebert, in the Ann. des Sci. Nat., xi, 1849, p. 349.
8. *Mikroskopische Anatomie, &c.;* von Dr. A. Kölliker. Leipzig, 1850, Bd. II, erste Hälfte, p. 199.

At this late period of histological research it may seem indeed a superfluous task to pass in review a subject apparently so well understood as that of the minute and ultimate structure of Muscular Tissue. But the truth is, that on this, as with most other subjects in microscopy, the advent of new observers, or the reappearance of old ones, in the field of research, bring with them the prestige of hitherto undiscovered facts and new truths, and so the old land-marks and positions laid down by earlier but by no means less able, faithful, and accurate observers, seem likely to be disturbed.

We do not propose here to enter upon any formal discussion of the historical relations of the doctrines advanced hitherto upon this subject. We desire to pass in review what we understand to be the leading features of the histology of this tissue, and

therein to seek the more or less definitely expressed *formula* of its structure—all of which will put us in a somewhat favorable position to regard critically some doctrines which are as remarkable as they are new.

But first of all we will briefly refer to some of the more prominent researches which, from time to time have served as true finger-posts as each succeeding investigator and explorer has passed along the road.

Although there can be but little doubt that some of those excellent old naturalists and observers of the last two hundred years, caught, with their rude magnifying powers, not erroneous glimpses of the complex intimate structure of muscle, yet any definite ideas of its real composition as explanatory of its mode of action, cannot be said to have been entertained until the days of the cell-doctrines of Schwann and Schleiden. It is true that since the time of Leeuwenhoek, or even, perhaps, before, it has been known that voluntary muscle could be split up into fine threads; but this was the limit of their real knowledge, for, if we except Fontana, none of the observers appear to have had distinct ideas of the nature of these threads.

Valentin, from studies upon the development of this tissue, had perceived clearly the general character of its composition, and Schwann, a few years after, applied more or less completely and successfully his cell-doctrine to its elementary formation and constitution. These undoubtedly were very important steps; but the contribution which marks an era in the histological history of this tissue is that of Mr. Bowman, which appeared in the Philosophical Transactions of 1840. Rare are the examples in the whole domain of minute anatomy, where so much real progress has been made by a single set of researches, as in this case. In more than one particular, Mr. Bowman exhausted the subject, and it is perfectly correct to say that in the leading and essential features of the minute anatomy of voluntary muscle, the numerous microscopical observers have added but little if any thing during the thirteen years that have since elapsed. Bowman's results are so well known and even familiar to all anatomists that it is almost out of taste to repeat them; but I will state them in a brief form: A series of discs succeeding each other in a row and at regular intervals; a row of discs thus formed constitutes the *primitive fibrilla.* Numbers of such fibrillæ are bound together with an exact coaptation of their discs and intervening spaces—constituting the striated muscular fibre. This fibre thus composed of a bundle of fibrillæ is encased in a special sheath, the *sarcolemma.* And, finally, a greater or less number of such encased fibres, bound together, constitute a *fasciculus*, and these fasciculi make up the gross muscle. We have then muscle: fasciculus, fibre, fibrilla, disc. Fibres may be split lengthwise, forming fibrillæ, and cross-

wise, forming large discs—the cleavage taking place, from the exact coaptation of the discs, through the light spaces.

Nothing can be more clear than the structure of a tissue thus wrought out, and with such data the student who has once caught its formula by an observation through the microscope on a good specimen, will never have his *idea* of striated muscle effaced,—for this in general is the elementary composition of voluntary muscular fibre wherever found, as a striated structure, among all animals.

Leaving for a future page a criticism of some of the points of Bowman's doctrine, we will continue our subject by some reference to the *development* of this tissue. Undoubtedly the most important because the most comprehensive researches that have been made in this direction are those of Kölliker and Lebert. The two leading features in the primary formation of muscle, are, first, that its origin is cellular, and second, that the fibre and not the fibrilla is the primary part evolved—the fibrilla being therefore a secondary or resultant formation.

The fibre is a more or less direct result of a fusion of a row of cells together:—this is the foundation; and the secondary changes which supervene thereon vary in extent and character according to the more or less complexity of the form of tissue ultimately evolved. The details of this genesis we need not here describe; all we wish to indicate is the original cell-constitution of muscular tissue in every locality where it is found; it might also be added that its departure from this original cell-condition and the metamorphosis of these cells into more or less complicated forms, holds a nearly corresponding ratio to the grade either of the animal in the scale of life, or of the function the particular tissue in question is to perform.

But these remarks, with the exception of the last general statement, refer particularly to the striated form of muscular fibre, to which the observations of Bowman and Lebert relate almost exclusively. But the other variety, the so-called smooth, non-striated fibres, is not the less interesting to the physiologist,—although they are, it is true, connected with the functions of the organic or non-voluntary life. Upon this subject, at least as connected with the higher animals, no single observer has thrown so much light as Kölliker. This excellent observer showed that the elements of the smooth or non-striated muscles, do not consist, as had hitherto been supposed, of long broad bands dotted with many nuclei, but are composed of comparatively short, isolated fibres, each of which contains a nucleus. The cell origin and constitution of these fibres (Faserzellen) are too apparent to be questioned. But these cells have experienced but few changes, and have undergone none of those elaborate alterations which supervene when similar cells form the striated variety of this

tissue. In one sense, then, the smooth, non-striated muscular fibres are only a kind of infra-formation of those which are striated—a condition of things in which the developmental changes persist on a low type. That this is a true version of these phenomena would seem to be indicated by the fact that in some instances these two forms of this tissue seem to run into each other, and in some insects the muscles of organic life of the alimentary canal, contain both striated and non-striated fibres.

From smooth, non-striated muscle we can proceed downward at least one step farther, where this tissue is cellular in its adult condition, a state of things where the developmental changes have been arrested, as it were, at the very outset. Muscular tissue of this simple kind is found in the Polypi and Acalephæ, especially, and contraction of muscles thus composed, occurs by a lateral widening of the cells, the whole muscle increasing in breadth proportionate to its diminution in length.

With these exordial remarks, in which we have desired rather to notice the general relations of the subject, than to be concise and explicit on special points—we will now turn and take up somewhat in detail some particular relations of this tissue, basing our remarks upon a set of recent investigations we have made under quite favorable circumstances. In this way we shall have occasion to refer to the other writings we have placed at the head of this article.

We will commence by an examination of the highest form or the striated muscle. Striated muscle does not occur lower in the animal scale than the Articulata;* at least, in the other divisions of the Invertebrata, viz., the Cephalopoda, the Cephalophora, the Acephala, the Annelides, the Turbellaria, the Helminthes, the Echinodermata, the Acalephæ,† and the Polypi, our own observations agree with those of others who have specially examined the subject, that we have never been able to perceive anything of the

* *Busk* (Transact. Microsc. Soc. London, ii.) has described and figured a striated form of muscular tissue as occurring in *Anguinaria spatulata* and *Notamia bursaria*. We have been unable, however, after considerable search upon many Bryozoa, among which were several species of *Alcyonella*, to detect anything like real striped tissue of this kind. But it should be mentioned that the descriptions of *Albman* (Rep. Brit. Assoc. for the Advancement of Sci., 1850, p. 318), who has recently studied this subject, would leave but little doubt as to the existence of the striated muscular tissue in the species of Bryozoa he examined, unless perchance this observer was deceived by a plication of the muscular tissue which occurs in these animals. See, in this connection, Comparative Anatomy, by Siebold and Stannius, Translat. &c., by Burnett, vol. i, § 29. note 1.

† In the Proceedings of the Edinburgh Physiological Society for July 23, 1853 (See Edinb. Month. Journal Med. Sc., Sept., 1853, p. 279), Dr. Cobbold is mentioned as having demonstrated striated muscular fibres in the umbrella of *Medusa aurita;* but this point needs further research, since Agassiz says, in speaking of the structure of the muscles of Medusæ; "With all the power of the best Oberhäuser Microscope, I have been unable to discover the slightest indication of striæ upon the muscular cells." See Contributions to the Natural History of the Acalephæ of North America, in the Memoirs Amer. Acad. Arts and Sc., 1850, N.S., iv, pt. ii, p. 239.

kind. But in the three classes of the Articulata,—the Crustacea, the Arachnoidæ, and the Insecta, and in all the classes of the Vertebrata, it forms by far the larger portion of the muscular system. Wherever found, it is invariably the same as to the formula of its development and constitution, and whatever may be said as to the interpretation to be put upon this or that appearance, there can be no doubt but that it is always capable of being resolved into the same primary and secondary elementary forms.

The best specimens of muscular tissue, for the careful, detailed, and successful study of its elementary constitution are found in the Articulata, or, to speak more definitely from our own experience, in the Entomostraca of the Crustacea, or in the Neuroptera and Diptera of the Insecta. Our best and most satisfactory observations have been made upon the alary muscles of the common Musquito *(Culex pipiens)*, and which we could particularly recommend to those who would follow this line of inquiry.

A muscular fibre is, as we all know, a bundle of fibrillæ, but the question is, What is the histological nature of a fibrilla?

Dr. Martin Barry's view, that it has a spiral structure—being composed of two threads twisted together in opposite directions—is well known to histologists. This view, in common with the one of a like structure of the elementary parts of the organic world generally, was advanced in the first of his writings we have above named on our list. We know of no paper of its pretension which has met with such universal disfavor among investigators. But the misfortune of the case is, that this view should have again been produced in a resuscitated form, and pushed to its ultimatum even with new statements, as has recently been done in the last two writings we have cited of this observer. The paper published in Müller's Archiv appears under the prestige of being wrought out in many of its most important points with a most excellent *Pflossel* microscope, and as a whole prepared under the eye of the venerable Purkinje.

In truth, it would appear that Mr. Bowman's excellent researches to which we have already alluded, ought to have so far settled any disputation on these points, as to render any critical examination of these anomalous doctrines wholly uncalled for. But in our late review of this whole subject, we have not been unmindful of this theory, and particularly so since excellent opportunities have occurred showing what we believe to be the true version of not unfrequent, singular, spiral-like appearances observed in this tissue.

In the first place, we will say, that with the advantage of the use of some of Mr. Spencer's best and most powerful lenses, employed with the most favorable illumination, we have given several protracted sittings to the investigation of the nature of Dr. Barry's alleged phenomena, with his figures before us, and using

a great number of very excellently prepared specimens of this tissue taken from various animals.*

But with all these conditions we have failed to detect any appearance in muscular fibrilla indicating its spiral structure as advanced by Dr. Barry. This has been the result of all our examinations notwithstanding single fibrillæ in both a relaxed and a contracted state, have been subjected to an amplyfying power of more than 1000 diameters.

On the other hand we have not unfrequently observed appearances of the striæ of the fibrillæ, which, at first, and especially when a power of not more than 300 diameters is used, look remarkably like a spiral formation; but, by doubling the amplification the illusion is dispelled, and the curious appearance is shown to be referable to a peculiar arrangement or a kind of dislocation of the disc-like elements of fibrillæ. By a little rough treatment of separated fibrillæ in water, these appearances may often be produced to almost any extent. It is not uncommon to see the spiral appearance run one way half the fibrilla, and then change and run the opposite the remaining half. But the effect is most delusive when the disc-like elements of the fibrilla are not only turned awry laterally in a regular manner, but are also slightly tilted up. These same changes taking place in a bundle of fibrillæ or a fibre, and occurring with a nice coaptation throughout, give to the whole fibre a very spiral-like aspect.† Other appearances than these, indicating a complicated spiral-composition of this tissue, we have never observed, and from what we have observed both of the development and of the mature structure of muscular fibre in all its forms, we can give no credence to the doctrines of Dr. Barry, and we say this without the least distrust of our instruments, specimens, and opportunities.

The view of Kölliker upon the composition of the muscular fibrilla, is, in one sense, the very antithesis of the one we have just noticed, for he thinks that it has no structure at all, but is a homogeneous formation. He says: "The fibrillæ are composed of a substance which, although but little, is yet very perfectly elastic, and is therefore capable, from mechanical influence, of a very considerable elongation and subsequent shortening. In the extended condition, they are smooth and thin; but, when contracted, they are

* The specimens of muscular tissue we have used in these examinations, were prepared for us by Dr. Durkee, of Boston. Dr. D. has a happy ingenuity in such things, and the specimens in question, for beauty of exhibition of the ultimate structure, as well as for the completeness of preservation, exceed by far anything we have before seen.

† *Aubert* in his account of the structure of the thoracic muscles of insects has also alluded to this point, and one of his figures very well represents the spiral-like appearance of the fibrillæ seen under a low power. See Ueber die eigenthumliche Structur der Thoraxmuskeln der Insekten, in Siebold and Kölliker's Zeitsch. f. wissensch. Zool., iv, p. 388. Taf. xv, fig. II, *b*, and IV, *b*.

thicker, and have varicosities at regular intervals. . . . My opinion is, that the transverse striæ hold no essential, internal relation to the contraction of the fibres, but are simply an expression of this last and of the organic elasticity of the fibrillæ."—(Mikroskop. Anat., loc. cit. p. 263, 264.)

This view which makes the fibrilla a homogeneous formation, and according to which the striæ and disc-like structure, as usually seen, are only resultant or secondary conditions, demands a special examination, considering the high authority of its source.* In the first place, we have not been successful in verifying the chief datum on which this opinion is founded; we refer to the complete disappearance of the striæ and the consequent perfectly smooth aspect of the fibrilla. If a small bit of muscle be taken fresh, from the thorax of a Musquito for instance, and be treated in water at the same time being severely handled by pressure, extension, &c., and then put under the microscope, the field will be seen filled with fibrillæ which are pale, and at first, appear perfectly smooth. The striæ seem to have entirely disappeared, and cannot be seen, we are satisfied, with an amplification of even 500 diameters, but using double this power, or 1000 diameters, together with the advantage of favorable light, the striated appearance is distinctly observed, although irregular and disturbed from the mal-treatment received. We have prepared and repeatedly examined many specimens of this tissue thus harshly handled, and in every instance where the fibrillæ preserved their usual fibrilla-conformation, we have not failed clearly to perceive the ordinary although perhaps distorted striated appearance.

Judging from our own experience, therefore, we should be disinclined to accept the opinion that fibrillæ may be made to appear free from striæ,—on the other hand, we should attribute the alleged smoth aspect to the fact that probably sufficiently high and excellent microscopic powers were not used in the examinations.

After this discussion of different and dissimilar opinions, we may revert to our primary leading question: What is the histological composition of the muscular fibrilla? We shall be greatly aided in satisfactorily answering this question by referring to these formations as occurring under their simplest and most naked forms. If the muscles of some of the lower Crustacea, of *Argulus* for instance, or what is more convenient, of the thorax of the common house fly (*Musca harpyia*), be taken and examined under the microscope with the usual care, the fibrillæ will be seen very distinct, and separated from each other, at the same

* *Hassall* advocates the same opinion based upon the allegation that the striæ may be made entirely to disappear by the action of acetic acid, (see The Micros. Anat. of the Human body, &c., vol. i, p. 341); but this has not been the result of our own experience.

time very fragile, for, with light pressure, they immediately break up into discs or small regularly-shaped particles. This fracture is very uniform, and the resultant particles are of such a complete character in themselves, that they may be studied as distinct objects exactly as blood-discs or other organic particles. Observed with a power of 1000 or 1200 diameters these separated atoms present a regularity of form, shape, and size, fully indicating that they are special formations, in contradistinction to the view that they are the results of a systematic although accidental fracture of a homogeneous cylinder or band.* From these examinations and investigations, as well also as from others of a collateral character, we advance the opinion of the true histological existence of the discs or regular particles composing the fibrillæ, and that the transverse striæ are due to the presence of these particles and the appearances resulting from the action on them of the light.

While therefore we would support Bowman in his doctrine of the real existence of the sarcous elements, yet contrary to this observer, we shall equally believe in the existence *per se* of the fibrillæ, instead of regarding them as purely accidental cleavage formations. The phenomena observed in the muscles of many of the Articulata and especially those to which we have already referred, seem to admit of no other construction. Again, we have seen no evidence that, as Carpenter and Sharpey have advanced, the particles or small disc-like bodies composing this fibrilla, are each a "minute cell."† This is a point that has been particularly looked to, with the use of the high and excellent lenses we have the good fortune to possess. In every case these disc-like particles have showed nothing indicating a cell-constitution, but have appeared as homogeneous, elastic bodies, quite like many other organic particles we have not unfrequently met with in histological investigations.

Thus far the subject has appeared clear: but we wish now to notice some points and appearances observed, the signification of which seems a little obscure, or at least not satisfactorily determined.

If the best examples of muscular tissue in the form of fibrillæ are examined,—for instance, specimens from the thorax of the musquito,—there may often be observed, besides the spiral-like arrange-

* Leydig has described and figured this separation into discs of the muscular fibrillæ, as occurring in *Branchipus*. His observations in this respect upon these Entomostraca correspond closely with our own upon many of the Insecta. See, Ueber Artemia salina und Branchipus stagnalis, in Siebold and Kölliker's Zeitsch. f. wissensch. Zool., iii, p. 280, Taf. viii, fig. 10, 11. This same observer also speaks of some muscles (those of the alimentary canal) of *Artemia*, being composed of spindle-shaped instead of disc-like elements, and so arranged, points and bases alternating, as to form a symmetrical fibrilla; see loc. cit. Taf. viii, fig. 6.

† See *Carpenter*, Princip. of Human Physiol., Amer. Ed., 1853, p. 305, and *Sharpey*, Quains Elements of Anat., 5th ed. ii, p. 168.

ment of the discs, anomalous appearances in the fibrilla which, although regular, are undoubtedly due to some abnormal changes occurring in the elastic substance of which the fibrilla or even its discs are composed. Several of these curious aspects have been described and figured by Mr. Dobie in his paper above cited. Thus, in some cases, instead of the appearance of a regular succession of square blocks which appear white, while the line of their apposition appears dark,—presenting, on the whole, a kind of tape-worm appearance,—there is observed another dark line bisecting the light space. There are then seen, broad dark line (the line intervening between the discs), light space, narrow dark line (line bisecting the disc itself), light space, and, broad dark line again. In other cases, the broad dark line is as wide as the disc itself, and then there appears a regular succession of light and dark spaces of equal width – the light spaces being bisected by a dark line, and the dark ones by a light line. But this beautiful series of light and shade may be completely reversed by a change of focus. In some rare instances, one of which we have now under observation, these light and dark spaces are quadrisected instead of bisected, there being two light or dark narrow lines instead of one across each space.

These singular appearances, of which there are numerous other varieties, might perhaps be accounted for on the supposition that, from rough handling, changes have occurred in the constitution of the component substance of the fibrillæ, which produce the refraction of the light here observed. But we will add that in all the examinations we have made of this interesting point, we have seen nothing in these varying aspects leading us to doubt the true disc-composition of the fibrillæ.

Another point to which we would here refer as at least of some suggestive interest, is the question whether the fibrillæ are invested with a sheath or not. Until quite recently we have observed nothing indicating that such sheath exists; but in some lately-prepared specimens, not only the disposition of the discs composing the fibrillæ, to each other, but also a kind of crenulated or denticulated aspect of the edges of the fibrillæ, sometimes observed;—such phenomena might be well explained on the supposition of the presence of a sheath, but, at the same time, they by no means demand this supposition. In some instances they have a striking appearance, but it is possible that they may be the remains of that gelatinous substance which binds the fibrillæ together in a fibre.*

* Dobie speaks of a homogeneous, delicate membrane, which is sometimes observed stretched between two fibrillæ when they are separated at a greater or less acute angle from each other, resembling the web between the toes of a palmipede bird. It may be the same substance that we have observed. See loc. cit., p. 114, Pl. VII, fig. 8, *a*, *b*.

As to the minute structure of the *non-striated muscular tissue*, we have but little to say. We have found it uniform and the same wherever occurring, even when it has the function of voluntary tissue, as is the case in the Mollusca and Radiata.

This last mentioned fact shows that the voluntary movements in animals are not necessarily performed through the agency of the striped fibre,—however much the constitution of this last would seem to indicate a greater nicety and coaptation of action, as we perceive is really the case. In no case have we observed it capable of being separated into fibrillæ, like the striated variety, as has been stated by Lebert and Robin to be the case in some of the Cephalophora*; on the other hand, in many species of this class, which have been examined we have found this tissue to be composed of fibres which admit of no further division except into their constituent cells.

Less studied, but at the same time more interesting, histologically, is that form of this tissue, which persists permanently on the cell-type, the cells still preserving their characteristics as such, instead of being lost in their contribution to a distinct form of tissue. This form of tissue composes the mass of muscle of the Radiata proper, and is perhaps best observed in the Acalephæ, and Polypi. Agassiz has given it a good description, as occurring in the former of these classes,† and in the latter we have frequently observed it, while making other investigations. A very fine example of this cell-muscle may be observed in the pedicle of the medusa-form of *Tubularia:* here, a double row of cells is observed;—these last, when in a state of relaxation are round or ovoid, but when contracted they wear a flattened, disc-like form, increasing the width of the pedicle in proportion to the contraction which has taken place. Nothing can be more beautiful to the physiologist than the observation of such phenomena, for nature is here observed with just that amount of drapery which would hide yet adorn her nakedness.

Before closing these remarks on the histological composition of muscle, we wish to refer to another point, in the form of an inquiry: Does not muscular tissue occur under even a lower form than that of being composed of cells? This query is advanced, because in some instances of the locomotive, contractile, if not muscular tissue of the lower animals, we have not succeeded in making out any cell-structure. This has been the case with the otherwise muscular, and highly contractile parts of some of the lower Helminthes, where the tissue in question seemed resolvable into granules alone, allocated under distinct forms by a delicate

* See, Kurze Notiz über allgemeine vergleichende Anatomie niederer Thiere, in Müller's Arch., 1846, p. 120.

† See Contributions to the Natural History of the Acalephæ of North America, in the Memoirs Amer. Acad. Arts and Sci., 1850, N. S., iv, P. II, p. 239.

fibrillated membrane. Again, in a long fusiform muscle of one of the tentacles of an *Alcyonella*, we have observed a like want of any cell-constituents—the muscle being apparently composed of a delicate granular, punctiform substance. On contraction, the muscle changed from fusiform to a fan-shape, its punctiform aspect becoming darker and more condensed. Further research upon these intricate points is greatly needed, but it should be remembered that there is, histologically, no reason why a granular stroma or substance should not possess contractility, as well as the elaborated formations from cells. It is true that its action may be more gross and less adaptive, but it may well possess an inherent elasticity which may be more or less under the control of the nervous system. As examples of this line of suggestion, may be cited the relation of cilia and other formations, which certainly have nothing muscular in their structure, although they possess a wonderful capability of motion, which is sometimes under the influence of the nervous system.

It is in this way that studies upon the ulterior constituents of organized forms, teach us that function in nature lies deeper than the material part in which it is manifested, or, in other words, that function cannot be regarded as the result of this or that material form, but rather that this last exists as such in virtue of the former being perfectly performed.*

In regard to the nature and character of muscular contraction—the concluding point in this discussion of our subject, it may be said that nothing is really known. We can observe the physical phenomena that attend the act of contraction and of relaxation in this tissue, but here is the limit of our successful inquiry, for the internal relation of this process is hidden from our perception, and is as much an enigma as the real essence, the remote cause, of everything else connected with the organic world about us.

The apparent phenomena in question are well-known and easily observed. A fibrilla contracts, and its increase in lateral bulk is proportionate exactly to the decrease in length that has taken place;—no space, therefore, has been gained. In the striated form of this tissue, this shortening is attended with an approximation of the constituent elements of the fibrilla—the discs; the striæ, therefore, appear in closer succession. In the two other forms of this tissue, the unstriated and the cellular, nothing is observed but a simple lateral increase of the fibres, which is precisely correlative with the amount of contraction. This much, therefore, may be said about the matter: Muscle, in virtue of its very organization as a peculiar tissue, possesses a particular organic

* See, for an inquiry into the nature of the contractile tissues of the lower animals, *Ecker*, Zur Lehre von Bau und Leben der contractilen Substanz der niedersten Thiere, in *Siebold* and *Kölliker's* Zeitsch. f. wissensch. Zool., i, p. 218. The views here advanced support those we have suggestively thrown out in the above remarks, as based upon the quite limited observations we have made on this subject.

property—*contractility*—a capacity by which its substance may be shortened or lengthened from the action of stimuli. The chief of these stimuli, is, of course, nervous force; but this last is very far from being the exclusive or necessary one, for, as we have often observed under the microscope, detached and wholly isolated fibrillæ may not unfrequently be seen to contract and relax from the stimulus of water and other liquids. We would, therefore, support the doctrine of Haller, in that muscle does not positively require for its action, the influence of a nervous system; but at the same, time we would not wish to be understood as advancing the doctrine of the true spontaneity of action with this tissue; on the other hand we would say that this action must always be the result of some stimulus, from within or without, which excites a manifestation of a peculiar organic capability.

Postscript.—Since the above was written and mostly printed, we have received the November No. of the Philosophical Magazine, which contains two recent communications by Dr. Martin Barry relating to our subject, "A Main cause of discordant Views on the Structure of the Muscular fibril," and "Further Remarks on the Muscularity of Cilia." These papers contain nothing but a reiteration of Dr. Barry's former doctrines under, what he would perhaps think, better auspices. But we have failed to perceive anything new, or indeed anything but a restatement of his previously advanced opinions; neither do we think that Dr. Barry has very satisfactorily stated the "main cause of discordant views" on the subject, although we do not think it doubtful wherein this cause really lies.

Record of Anatomy and Physiology, Dec. 1, 1853; by W. I. Burnett.

SPECIAL WORKS.

Friedreich, (*D. N.*) Beiträge zur Lehre von den Geschwülsten innerhalb der Schadelhohle. 8vo.

Klencke, (*Prof. H.*) Mikroskopische Bilder Naturansichten aus dem kleinsten Raume. In Briefen an Gebildete. Mit 430 Holzschn. 8vo.

Müller, (*J.*) Ueber den allgemeinen Plan in der Entwickelung der Echinodermen. Mit 8 Tafeln. 4to.

Bock, (*C. E.*) Lehrbuch d. pathol. Anatomie u. Diagnostik. Bd. II, Abth. 2. 8vo.

Heusinger, (*D. C. F.*) Recherches de Pathologie compareé. 2 vol. 4to.

Engel. Die Entwickelung rohrige u. blasiger Gebilde im thierischen Organismus. Mit 2 Tafeln. 8vo.

Canstatts, (*C.*) Specielle Pathologie u. Therapie vom klin. Standpunkte aus bearbeitet. 3te Aufl. Von Dr. E. Henoch. In 2 Bdn. od. 8–10 Lief. 1 Lief. 8vo.

Kehrer, (*Dr. F.*) Das Blut in seinem Krankhaften Verhältnissen: Ein Beitrag zur Pathogenie. 8vo.

Carus, (*C. G.*) Tabulæ Anatomiam Comparativam illustrantes quas exhibuit. Carolus Gustavus Carus junctus cum Eduardo D'Altone. Pars VIII. 4to.

The Transactions of the American Medical Association. Vol. vi, pp. 832.—Besides the details of the business of the Association, the reports of the various committees, and Essays on particular medical subjects, this volume contains two

prize Essays—one on The Surgical Treatment of certain fibrous Tumors of the Uterus; by Washington L. Atlee, M.D., of Philadelphia—the other on The Cell: its Physiology, Pathology, and Philosophy; as deduced from original investigations, to which is added its History and Criticism; by Waldo I. Burnett, M.D. of Boston.

As for the other or first mentioned of the above works, our space does not allow us to furnish, at this time, a special or even a general description of their contents.

II. PERIODICAL LITERATURE.

THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW. Oct., 1853.

The present No. of this well known and high-toned Journal is one of more than usual interest in a physiological point of view. It opens with an able and very agreeably written article by T. H. Huxley, on the Cell-Theory—also other able contributions on subjects correlative to practical medicine, and particularly the well-digested Annals of Micrology by Dr. Lyons—a labor of great value, since few, comparatively, can have access to the numerous foreign works and periodicals, from which Dr. Lyons so successfully culls. We hope to have the opportunity soon to discuss some points in Mr. Huxley's article in relation to cell-life and organization.

MÜLLER'S ARCHIV FÜR ANATOMIE, PHYSIOLOGIE UND WISSENSCHAFTLICHE MEDICIN. 1853. Heft. 3, 4. August, September.

Max Schultze. Ueber Chætonotus und Ichthydium (Ehrb.), und eine neue verwandte Gattung Turbanella.
A. Krohn. Ueber die Larve von Spatangus purpureus.
August Müller. Beobachtungen zur vergleichenden Anatomie der Wirbelsaüle.
A. Krohn. Ueber die Entwickelung der Seesterne und Holothurien.
Ed. Grube. Ueber den Bau von Peripatus Edwardsii.
A. Krohn. Ueber die Larve von Echinus brevispinosus.
H. Meyer. Das aufrechte Gehen. (Zweiter Beitrag zur Mechanik des Menschen Knochengerüstes).
Adolph Fick and *Paul du Bois-Reymond.* Ueber die unempfindliche Stelle der Netzhaut im menschlichen Auge.
L. Fick. Beitrag zur Temperaturtopographie des Organismus.
A. Krohn. Ueber die Brut des Cladonema radiatum und deren Entwickelung zum Stauridium.
W. Peters. Ueber des Kiemengerüst der Labyrinthfische.
W. Berlin. Ueber einem Wurm aus der Gruppe Anguillulæ, Enophies quadridentolus.

ANNALES DES SCIENCES NATURELLES, xix, 1853. Nos. 4, 5.

Duvernoy. Mémoire sur les Oryctéropes du Nil blanc ou d'Abyssinie, et du Sénégal, suivi de nouvelles recherches sur la composition microscopique de leurs Dents. p. 193.
Lacaze Duthiers. Recherches sur l'armure génital femelle des Insectes lépidoptères. (Suite.) p. 203.
—— De l'armure genitale des Aphaniptères. p. 213.
—— De l'armure génital des Insectes en général. p. 215.
Bischoff. Recherches sur la production de l'Urée. p. 238.
Camille Dareste. Analyse des Observations de M. Müller sur le développement des Echinodermes. p. 244, 257.
Claude Bernard. Recherches sur une nouvelle fonction du Fois, considéré comme organe producteur de matière sucrée chez l'homme et les animaux. p. 282.

COMPTES RENDUS, xxxvii, from August 8, to October 3, 1853.

A. Lavocat et *N. Joly.* Études anatomiques et tératologiques sur une mule fissipède aux pieds anterieurs. p. 337.
Philipeux et *Vulpian.* Mémoire sur la structure de l'encéphale des Poissons cartilagineux et sur l'origine des nerfs craniens chez ces Poissons. p. 341.
Lacaze-Duthiers et *Riche.* Recherches sur l'alimentation des Insectes gallicoles. (Report on, by Quatrefages). p. 394.
A. Kölliker et *H. Müller.* Note sur la structure de la rétine humaine. p. 488.

Virchow. Déconverte d'une substance qui donne lieu aux mèmes réactions chimiques que la cellulose végétale, dans le corps humain. p. 492.
Gegenbaur. Recherches sur le mode de reproduction et sur le développement dans divers groupes de Zoophytes et de Mollusques. p. 493.

THE ANNALS AND MAGAZINE OF NATURAL HISTORY, vol. xii, October, November, 1853.

Thos. Williams. On the Mechanism of Aquatic Respiration and on the Structure of the Organs of Breathing in Invertebrate Animals. p. 243.
Clark. On the Branchial Currents in the Bivalves.
J. E. Gray. On the Teeth of the Pneumobranchiate Mollusca.
Thos. Williams. On the Mechanism of Aquatic Respiration, and on the Structure of the Organs of Breathing in Invertebrate Animals.

THE QUARTERLY JOURNAL OF MICROSCOPICAL SCIENCE, Nos. 4, 5. July, October, 1853.

[Transact. Microsc. Soc., London.]

Williamson. On the minute Structure of a species of Faujasina.
Gregory. Notes of a Diatomaceous Earth found in the Isle of Mull.
Wheatstone. On the Binocular Microscope and on Stereoscopic pictures of Microscopic Objects.
Wenham. On the application of Binocular Vision to the Microscope.
Shadbolt. A short description of some new forms of Diatomaceæ from Port Natal.
Legg. Observations on the Examination of Sponge Sand, with remarks on collecting, mounting and viewing Foraminifera as microscopic objects.
Rainey, (Geo.) A method of employing artificial light for the illumination of transparent objects, by which it is so deprived of glare and color as to be equal in its illuminating power to the best day-light.

[Original Communications.]

Brightwell. On the genus Triceratium, with descriptions and figures of the species.
Salter. On certain appearances occurring in dentine, dependent on its mode of calcification.
Lister. Observations on the Muscular Tissue of the Skin.
T. H. Huxley. On the Structure and relation of the Corpuscula tactus, and of the Pacinian bodies.
G. Rainey. Some observations on the illumination of transparent objects.
Herapath. Paper on the discovery of Quinine and Quinidine in the Urine of Patients under medical treatment with the Salts of these mixed alkaloids.
Riddell (of New Orleans, U. S.) On the Binocular Microscope.
Gregory. Additional observations on the Diatomaceous deposit of Mull.
Here follow short but interesting translations from the continental Journals, and Reviews.

PHILOSOPHICAL TRANSACTIONS, 1853, Pts. 1, 2.

Hanfield Jones. Further Inquiries as to the Structure, Development, and Function of the Liver.
T. H. Huxley. On the Morphology of the Cephalous Mollusca, as illustrated by the Anatomy of certain Heteropoda and Pteropoda collected during the Voyage of H. M. S. "Rattlesnake," in 1846–50.
J. Tomes. Observations on the Structure and Development of Bone.
Newport. On the impregnation of the Ovum in the Amphibia, (Second Series, revised.) And on the direct Agency of the Spermatozoon.

ART. X.—*Biography of Berzelius;* by Prof. H. ROSE, of Berlin.

(Concluded from vol. xvi, p. 313.)

THE next subject to which Berzelius turned his attention belongs to organic chemistry. It was a comparative investigation of tartaric and racemic acids. He first corrected his former analysis of tartaric acid, in which he had given an atom more of hydrogen than Prout and Hermann, and adopted the results of these chemists. But he then found that the crystallized tartaric acid had precisely the same composition as the effloresced racemic acid, and that both acids had the same capacity of saturation,—facts which, especially at that time, were in the highest degree remarkable. This was one of the first clearly demonstrated examples that bodies of different characters may have the same composition. Berzelius had, sometime before, observed a somewhat similar fact in reference to the oxyds of tin, and Faraday, a short time afterwards, in reference to the compounds of carbon and hydrogen. Clarke had also discovered the remarkable modification of phosphoric acid, which he called pyrophosphoric acid. On this occasion Berzelius combined together, in an interesting manner, what was known of these bodies, to which he gave the name Isomeric. This term has been universally adopted, now that the number of such bodies has been so greatly increased.

From this time Berzelius frequently occupied himself with subjects which are certainly of the greatest interest to every thinking chemist, and indeed for every scientific man, since they are calculated to unfold to us somewhat more fully the nature of matter. He made known his views on this subject repeatedly, both in his "Jahresberichte," and in the several editions of his "Lehrbuch." Finally, he assumed two essentially distinct kinds of isomerism, and, in the strictest sense of the word, called those bodies only isomeric in which the elementary atoms may be regarded as grouped in different ways, forming compound bodies. These isomeric bodies may again be of two different kinds. They consist either of compounds which, with equal atomic weights, present different characters, or of compounds in which, though they possess different characters, the relative proportion of the constituents is the same, but in which the atomic weights are not equal, but twice, thrice, etc., as great as that of each other. Such bodies Berzelius termed, for the sake of antithesis, Polymeric compounds.

The other kind of isomerism Berzelius called Allotropism. It refers solely to elementary bodies, which, owing to causes not yet sufficiently understood, assume a different character from that which is usual to them, and, as it appears, retain this difference in

many combinations, where it may be the cause of differences in the character of these compounds. When isomeric conditions are observed in compound bodies, which consist of only two elements, combined in very simple proportions, this isomerism is, according to Berzelius, to be regarded less as owing to the different arrangement of the elementary atoms than to the allotropic condition of one or both of these elements; nevertheless, instances may occur in which both causes are simultaneously at work.

It is possible that Berzelius may sometimes have gone too far in his assumption of allotropic conditions, for there are some grounds for believing that an apparent allotropism may result merely from a different state of division. Thus, a few years before the discovery of the first example of isomerism, Magnus observed the interesting fact, that when the oxyds of iron, nickel, and cobalt, are reduced by means of hydrogen to the lowest possible temperature, the metals obtained ignite spontaneously, and oxydize when exposed to the atmosphere. This pyrophoric character evidently results from the finer subdivision of these metals, and it is destroyed when a higher temperature is employed in their reduction, which causes the particles to cohere together. The differences in platinum, according as it is reduced from its salts by the humid process, or obtained by igniting the ammonio-chlorid, and likewise the unequal combustibility of silicium, and its variable solubility in hydrofluoric acid, may probably be explained in the same way. Nevertheless, Berzelius was inclined to ascribe all these differences to allotropic conditions.

Shortly after the appearance of the paper in which Berzelius treated of bodies which, with the same composition, have dissimilar characters, Dumas went so far as to put forward the bold question, Whether many elementary bodies were not allotropic conditions of one substance, especially such as have the same, or very near the same, atomic weight, as nickel and cobalt, platinum and iridium, &c.? Berzelius favored this hypothesis, and regarded it as befitting, that new ideas should be followed up in all directions, even when it is not possible at the same time to adhere strictly to that which is, for the moment, to be regarded as probable; for truth sometimes appears to be inconsistent at the first glance, and in any case this was a way to arrive more rapidly at the results which might follow from a new idea. Certainly, upon the other hand, it cannot be denied that the question respecting a relation similar to isomerism between elements which have analogous but still distinctly different chemical characters, belongs to a domain, where perhaps our conjectures will never admit of being put to the proof.

The next paper by Berzelius was upon Vanadium. Sefström had found a new metal in the bar-iron of Taberg, which he called by this name. He had, however, restricted his investigation

to the preparation of the oxyd, or rather the acid of this metal, from the finer slags of the Taberg iron, and the determination of its distinguishing characters. He then transferred his stock of vanadic acid to Berzelius, in order that he might investigate the characters and history of the new metal. This investigation is a very extended one, and through it we have become acquainted with the new body in all its relations; whilst, as these are manifold and interesting, and as the acid has but little resemblance to other acids, it was difficult to assign to it its true position among them. In this respect the paper of Berzelius on vanadium may almost be compared with that upon selenium; for both have this peculiarity in common, that by them we have become so thoroughly acquainted with new and hitherto entirely unknown bodies, although in both instances but very minute quantities of rare material could be employed, that subsequent investigations have added but little to our knowledge, and nothing essential. Vanadium was afterwards found at several places, although always in very small quantities. Wöhler directed especial attention to the fact, that the acid of the new metal was contained in the lead ores of Zimapan, in Mexico, in which, as early as 1801, Del Rio discovered a new metal, and called it Erythronium; but misled by the authority of Collet-Descotils, who declared it to be chromium (with which Vanadium has certainly some similarity,) he afterwards admitted that his discovery was an error.

His next researches, which were upon Tellurium, were of a similar nature. Berzelius had already instituted experiments with very minute quantities of this metal, in so many respects interesting, but he was compelled to discontinue them for want of material. When Wöhler sent him a considerable quantity of this rare metal, which he had prepared from the telluric bismuth of Schemnitz, he again commenced the investigation. He first shewed how this metal can be prepared in its purest state. He then prepared all the compounds of tellurous acid (peroxyd,) as well as telluric acid, discovered by him, with bases, and indeed the different isomeric modifications which these acids form. These researches were likewise so complete, that they fully developed the history of this remarkable metal in all its relations.

The last great investigation by Berzelius, is that upon meteoric stones. He undertook this with the intention of studying these bodies, (as my brother and Nordenskjöld had already done,) as species of rocks, and, by this means, to determine what individual minerals they contained. The immediate inducement was a meteoric stone sent to him by Reichenbach, which had fallen a year previously in Moravia. But besides this, he examined three other earthy meteoric stones, and two masses of metallic iron. Berzelius inferred from his analysis that meteoric stones consist

entirely of such minerals as are found upon the earth, and that they certainly do not contain any elementary constituent which is not met with in terrestrial bodies. It was only in the meteoric stone of Alais that he found carbon in an unknown state of combination: this stone, when placed in water, disintegrated and fell to powder, which had a mixed smell of clay and hay. This shewed that if, as Berzelius considered, meteoric stones originated from other cosmical bodies, in their native state they could be converted into clayey mixtures, like the rocks on our own globe. He then raised the question as to whether this carbonaceous earth from the surface of another cosmical body contained organic remains, and consequently, whether there were upon its surface organised bodies, more or less resembling those on our earth? It is easy to conceive the interest with which he attempted to solve this question. This solution was not affirmative, but the results of his experiments did not justify a negative inference. Water and alkalies did not extract anything organic from the meteoric mass; on dry distillation, however, carbonic acid, water, and a blackish-grey sublimate were obtained, but no empyreumatic oil and no hydrocarbon; the carbonaceous matter was, therefore, not of the same nature as the humus on the earth's surface. The sublimate heated in oxygen, gave no carbonic acid or water, and changed into a white insoluble substance, whose nature could not be determined on account of the minute quantity. But to have pronounced it to be an elementary body, not originally belonging to our earth, would have been unwarranted.

This was the last extensive research made by Berzelius. His health, which, never strong, had already often necessitated the interruption of his labors, became, with increasing age, more delicate, and no longer admitted of his remaining continuously in the laboratory. He suffered, as is not unfrequent with intellectual men, especially from nervous headaches, which could not be mitigated by the most moderate living. He now begun to complain of a failing of the senses, especially his sight, and also of the weakness of his memory.

But his scientific activity did not on this account cease. He interested himself to the last for every branch of chemistry, and took the most active share in all the achievements of this science. Indeed, now that he was no longer occupied by important practical labors, he concentrated his activity more especially upon undertakings of a literary character, and with a zeal and industry which deserve the greater acknowledgment, since his bodily sufferings increased every year.

Among the products of the literary activity of Berzelius, I will here only make especial mention of the different editions of his "Lehrbuch der Chemie," and his "Jahresberichte ueber die Fortschritte der physikalischen Wissenschaften." His other works,

the lectures upon Animal Chemistry, and his work on the Blow-pipe have already been spoken of.

The "Lehrbuch der Chemie" first appeared in Swedish. It was translated into German first by Blumhof, then by Blöde and Palmstedt, and the later editions were translated by Wöhler and Wiggers. It was also translated into other languages, but did not pass through so many editions in any, as in the German, for besides the translations of Blumhof and Blöde, five editions have appeared. The last but one, the fourth, consisted, on completion, of ten parts. The fifth and last was commenced by Berzelius in 1842, but was not completed, only five volumes having appeared, certainly very large, each one containing nearly sixty sheets. The inorganic chemistry alone was completed. Of the organic part contained in the last two volumes, the most important—the animal chemistry—is wanting.

In this work Berzelius has treated very fully of all the facts appertaining to the science, with remarkable clearness, perspicuity, and apt illustration. At the same time, every subject is criticised in such an impartial and just manner as can be displayed only by one who stands as high in science as he did. The arrangement which he selected is indeed not a strictly systematic one, which, in a science so imperfect as chemistry, can certainly only be called convenient. But especially in the inorganic part, there is still a certain well-founded succession, such that it is very easy to become familiar with the work. In the organic part the facts are not arranged according to a strict scientific principle, and a classification adapted for inorganic compounds could not possibly be carried out with organic bodies. For although Berzelius had always declared himself strongly in favor of the application to organic chemistry of what we know of the modes of combination of the elements in inorganic nature as the clue by which alone we could arrive at a knowledge of organic bodies, still he was compelled to admit, that we were far from having advanced so far as to be able to treat of all organic bodies as radicals, oxyds, chlorids, &c., as in inorganic chemistry. Most of the assumed organic radicals, often of a complicated nature, are of a hypothetical nature; they gain a character somewhat certain only when some compounds of the radical with other simple radicals can be produced, and the oxygen in them replaced by chlorine, sulphur, &c. In addition to this, chemists are of very different opinions as to how the composition of organic bodies is to be represented, even when they agree in a fundamental principle. Moreover, as is natural, the different arrangements vary, according as more new facts are discovered. For the present, therefore, it is at least more advantageous to treat of organic bodies in an elementary work in such a way as Berzelius has done, namely, in groups containing those bodies which have the greatest general

similarity in chemical characters. It has frequently been seen, that works in which a theoretical principle has been strictly followed throughout, do not so well fulfill their principal object.

In the organic part of this work, Berzelius has declared himself against the so-called substitution theory, and the law of types. He assumes, on the contray, that conjugate compounds exist in organic bodies, in which, for instance, acids are united with compound radicals, or with their oxyds, chlorids, &c., in such a way that the acid is not saturated, but is still capable of combining with bases without separation of the associated substance,—the conjunct,—which enters with the acid as a constitutent of the salt. When an acid has entered into such a conjugate combination, it has generally acquired such altered characters, that neither the acid nor its salts are similar to the free acid and its salts. When hydrogen is replaced in an organic substance by chlorine, or another halogen, this generally takes place in the conjunct and not in the acid, and the former does not on this account cease to play its former part, of modifying the character of the salts into which it enters, with its acids, more or less, and according as its composition is altered by substitution.

It has been asserted that the replacement of hydrogen by chlorine, in organic compounds, was not to be explained at all in accordance with the electro-chemical views of Berzelius, and that consequently these views were incorrect. But when such a substitution takes place, it is, as already mentioned, generally only in the compound radical,—that is, the conjunct, and a new radical is thus formed, in which chlorine may perhaps occupy the place of hydrogen, but cannot play the same part as it did. Substitution of elements may therefore be very satisfactorily explained, according to the principles of Berzelius; and if his theory be impartially compared with the others which have been put forward in such number in organic chemistry, the inference will be, that in the present state of the science it is in a position to explain the facts more satisfactorily than any other.

On looking carefully through the various editions of this work, it is impossible not to regard it with admiration. It is not only the clear and comprehensive description, which attracts,—the sound, impartial criticism, which compels men of opposite opinion to appreciate justly,—or the great minuteness which has not left unnoticed a single fact, however trifling, if it was of any influence—but it is also the enormous industry which must create astonishment. A scientific man who had done nothing more than publish this excellent work, in so many editions, each of which was so completely revised that but little of the previous edition was retained, could not be refused by us our grateful acknowledgments of his great industry: and yet this constitutes but a fraction of the achievements of Berzelius.

It is touching to call to mind the words with which he concluded the preface to the last German edition, which he could not quite complete; it is dated November, 1842. He says, "I cannot overlook that, even if the Almighty should grant me life and power to complete the edition of which the first part is now published, this will be the last. For this reason, I considered it necessary to revise it so thoroughly, that I could express the final views which have appeared to me as the most probable during the long space of time in which I was so fortunate as to be able to follow with uninterrupted attention the development of the science, from the first growth of the antiphlogistic chemistry up to the present time—fortunate if, among the many views which a future extended experience will alter or correct, at least some few may prove to have been rightly conceived. With the profoundest conviction of the uncertainty of our theoretical views as well as of their indispensability, I have endeavored, in presenting them to the reader, not to inspire him with any more firm conviction of their accuracy than they appear to me to merit, and I have therefore always directed his attention to the uncertainty in the selection of modes of explanation. It is a great obstacle to the progress of science to attempt to cause conviction of the truth of that which is uncertain. What is believed is not submitted to any further examination; and the history of science shews that a deeply-rooted belief in theoretical conceptions has often withstood the most palpable proofs of their inaccuracy. Many of the defenders of Phlogiston required a regular development of the doctrines of oxydation in order to be convinced of its truth, and many distinguished men died believing in Phlogiston."

An undertaking by no means less gigantic than his "Lehrbuch" was the publication of the "Jahresberichte," which appeared regularly from the year 1820 until the death of Berzelius. The last completed volume comprises the discoveries of the year 1846. Berzelius therefore published twenty-seven volumes.

After Berzelius had been elected, as successor of the botanist, Olaf Swartz, to the office of perpetual secretary of the Academy of Sciences, besides making other important changes which he considered necessary in the statutes of the Academy, he succeeded in carrying into effect the arrangement that annual reports on the progress made in the various physical sciences should be written by members of the Academy, especially the different curators of the Natural History collections of the Academy, and that these reports should be presented at the annual public meeting held upon the 31st of March, the anniversary, and extracts read from them, after which they should appear in print. Members of the Academy undertook to write such annual reports in the departments of Botany, Zoology, and Astronomy, Mathematics, and Technology. Berzelius himself undertook the reports on Physics,

Inorganic Chemistry, Mineralogy, Vegetable and Animal Chemistry, and Geology.

It was only a man like him, who as it were surveyed at one glance the whole range of chemistry, and himself worked so much in all its branches, that could have adequately executed such an undertaking. These reports will long remain an example of the way in which such productions ought to be carried out. They were very comprehensive in those departments with which Berzelius was most intimately acquainted,—inorganic chemistry, chemical mineralogy, and vegetable and animal chemistry; less so in the other parts, which contained only the most important discoveries in those sciences with which Berzelius had not especially occupied himself, or which he had not pursued during the latter half of his scientific career, such as physics and geology. The reports were generally objectively written. If the views of the author of the original paper corresponded more or less with those of Berzelius, he gave an abstract, proportionate in extent to the importance of the subject, but always most admirable. If, on the contrary, their views differed from his, he allowed himself to express his opinion upon them, and observed a noble and impartial criticism, which rarely became at all violent. In this respect, it is certainly to be regretted that precisely his last "Jahrbericht" closes with an energetic attack upon another celebrated chemist. But Berzelius never mixed up personalities with his judgments; and if sometimes one could not agree with them, still they were always of such a nature, that although they occasionally gave pain to those upon whom they were passed, they could never excite any bitterness.

For the science itself these reports were of the greatest value. Berzelius, on several occasions, drew from the investigations of others important conclusions, which had entirely escaped the notice of their authors; and as frequently did he direct attention to new experiments which should be made in order to strengthen the results already obtained, or upon which to found new arguments. In this manner he exercised a very beneficial influence. He was also led to make experiments himself by these reports; and he then gave their results, when they contradicted, improved, or extended those of others, in the reports.

These reports were especially long when it was necessary to refute opinions and views which Berzelius considered as detrimental to the progress of science. Thus, the reports of the discoveries of the year 1838 and 1839 contain very detailed arguments against the hypothesis that all organic acids are hydrogen acids, and against the substitution theory. These arguments have always a rare clearness and simplicity.

The objection has often been made to this report, that it was sometimes very complete, and in some instances too extended;

sometimes, on the contrary, especially in the physical part, scanty and imperfect. This is certainly true: but it was very natural that Berzelius should have a partiality for the treatment of those subjects in which he especially interested himself and of which he was most master; but as he was almost equally at home in all parts of chemistry, this objection cannot be made to the strictly chemical parts of the reports. With regard to the physical part of the reports, Berzelius had only undertaken it because no other member of the Academy would or could do so. It was only in the years 1838 and 1839 that the report was written by Von Wrede. As Berzelius had only occupied himself with those parts of physics which were intimately connected with chemistry, it is almost only these parts which are touched upon in his reports.

In the same way, there was no other reporter to be found for the geological part; but as Berzelius had never occupied himself specially with geology, and only in so far as it was connected with chemistry, he treated only of the chemical part of that science in his reports, and otherwise noticed only the geological researches referring to Sweden. In the latter volumes reports upon geology are altogether omitted.

I have thus attempted to furnish a sketch of the comprehensive scientific activity of Berzelius. It is probably seldom that science is so greatly advanced through the labors of one man, and there is scarcely any chemist who has furnished such admirable and sound contributions.

This representation of his scientific merits would, however, give only a feeble idea of the whole greatness of the man, were we to judge from it alone. It is rare that so perfect a correspondence of mind and character is found in any man. That which so irresistibly attached those who had the happiness to have any long intercourse with Berzelius, was not merely the lofty genius visible in all his researches; it was not merely the clearness, the astonishing copiousness of ideas, the untiring care, and the great industry: the general impression which he made was that of the highest perfection. It was—and every one who knew him intimately will agree with me—it was at the same time those characters which placed him so high as a man; it was the consideration for others, the noble friendship which he evinced towards all whom he considered worthy of it, the lofty disinterestedness, the extreme conscientiousness, the perfect and just recognition of the merits of others; in short, it was all those traits together which spring from a worthy and honorable character. These were the sentiments which inspired all those who for a longer or shorter time came into contact with him, and especially his pupils—of whom our Academy contains more than all the rest of Germany—with the most pious respect for his memory.

Berzelius travelled the path of Science together with other distinguished men, who likewise advanced chemistry with giant steps. This was a time such as no other science has yet known, for no other has grown up from its childhood to a certain maturity in so incredibly short a space of time.

Berzelius was born almost in the same year as H. Davy and Gay-Lussac. However similar were the labors of these three men in science, they were in other respects very different.

Davy's brilliant discoveries, especially that of the metallic nature of the alkalies, gave chemistry an extraordinary impulse, and caused great enthusiasm in its pursuit. He achieved great things by his discoveries, the further following out of which, however, he left to others. He died in the prime of life; but in a certain degree his intellectual blossom was already past. Born poor, he had attained to great honors and great riches, which were perhaps obstacles to his being subsequently as active for science as formerly. It is, moreover, in the highest degree to be regretted that, in the latter years of his life, his very extraordinary talents were entirely enstranged from that science for which he might have achieved so much.

Gay-Lussac commenced his scientific career with the discovery of an important law in physics, but he afterwards applied himself wholly to chemistry, and advanced it as much by accurate investigations as brilliant discoveries. To him is owing, among other important facts, the law, so important for the doctrine of definite proportions, that gases unite in simple relations of volume,—a discovery, however, of which he did not at first make the many applications that were possible. But the most brilliant researches of Gay-Lussac are indisputably,—besides those published in common with Thénard on physico-chemical subjects,—the two sets of researches upon cyanogen and iodine. Even independently of the extremely importance influence which these researches exercised upon the whole range of chemistry, they may be regarded as models of investigation, both as regards the total results, the strict consistency of the reasoning, and the admirable description. As often as they are read, even at the present day, they will be regarded with astonishment.

But when, soon after the appearance of his paper upon cyanogen, Gay-Lussac undertook, in conjunction with Arago, the editorship of the "Annales de Chimie et de Physique," his scientific activity became gradually less. The first volumes of this Journal certainly contain several small papers and remarks which call to mind the author of those on Iodine and Cyanogen; but after a few years he ceased to write almost altogether; and it is perhaps more to be sincerely regretted than in the case of Davy, that Gay-Lussac, who died but a short time since, and after Berzelius, should already in the vigor of life have renounced his active scientific career, which seemed to promise so much.

It was not so with Berzelius. He also, after years of poverty, gradually attained, if not to great wealth at least to external honors, without having sought them in the least. But these could not estrange him from science; on the contrary, he took advantage of every higher position for its benefit. Science was always the sole object of his endeavours, and he never employed them for a purpose foreign to it. So completely was his whole life dedicated to science, that, even under the sufferings resulting from a painful disease during his latter years, his whole thoughts remained bent upon it alone.

Such men present in their inspired labors, as it were, the type of the true man of science; and who does not feel himself happy to meet them in life?

ART. XI.—*Correspondence of M. Jerome Nicklès, dated Paris, October* 30, 1853.

OBITUARY.—FRANÇOIS ARAGO.—Death has recently made grievous inroads into the ranks of French science. We have seen the fall, successively, of Laurent, Auguste de St. Hilaire, the Botanist, Adrien de Jussieu, the last male descendant of the brilliant dynasty of the Jussieus, who died in July last, President of the Academy of Sciences, and member of the Botanical Section. A loss, still more recent, has increased this list of the dead—a loss irreparable, for it is that of a man, who was at the same time an illustrious philosopher, a champion of popular progress, and a distinguished citizen.

François Arago was born on the 26th of February, 1786, at Estagel, a small village of 3000 inhabitants, situated near Perpignan (Eastern Pyrenees). His father was Treasurer of Perpignan. With a moderate patrimony, and a numerous family, he could not give his children a liberal education; but Madam Arago was able to supply it, and devoted herself to their instruction; and she afterwards had the richest recompense which a mother can look for: her sons were all men of distinction. Besides François, who immortalized himself by his discoveries, we see Jacques and Etienne, who are distinguished in literature; Jean and Joseph, who were brave officers in the Mexican service; and last, Victor Arago, the youngest, now commandant of the Artillery.

The appearance of François Arago on the arena of Science was most opportune. His father had destined him to the law; but the young man had other tastes. He met, one day, an officer of engineering drawing a plan on the ramparts of the city, and enquired of him how he could obtain the right of wearing so fine a uniform. "Become a scholar of the École Polytechnique," was the reply. From that time the career of the young man was determined. Having no instructors, he gave himself to books, and in 1803, at the age of 17 years, he entered this National School.

At the end of a year he had left behind him all his fellow students, and was detached by Monge to the Observatory of Paris, where he commenced his researches in Physics and Astronomy.

In 1806, he left for Spain, where he continued under the direction of M. Biot, the measure of the meridian of France, which had been interrupted by the death of Mecham. On the demand of the National Convention which established the Decimal system, Delambre and Mechain undertook the measurement of an arc of the meridian between Dunkerque and Barcelona. It was this measurement that MM. Biot and Arago continued to the Balearic Islands. This journey in Spain was full of dramatic incidents to Arago. Encamped on the summits of the elevated peaks of Catalonia, our observer had to contend in turn with the wind, the cold, and hunger, and also with brigands, the chief of whom ended by becoming the Protector of our young savants.

A year after their departure for Spain, MM. Biot and Arago had nearly completed the measurements as regards Spain. The former then returned to Paris, and Arago went on to Majorca to continue his operations. But the war was on the point of breaking out between France and Spain; and the night signals, the instruments, and the movements of the young Frenchmen who remained at work about the summit of Galatzo, rendered him an object of suspicion to the Majorcans, and Arago was arrested and thrown into the citadel of Belver. He managed to escape, and embarked with his papers and instruments for Algiers. The French Consul made him reëmbark for Marseilles, but at the moment of entering the Gulf of Lyons, the vessel was captured by a Spanish corsair and conducted to Rosas; Arago and his companions were at first imprisoned and then thrown intn the Pontoons of Palamos. At last, through the reclamation of the Dey of Algiers, to whom the vessel belonged, Arago and his associates were returned to Algiers. But a revolution had there taken place, and the Dey had just been decapitated; the new Dey was unwilling to let Arago go away, whom he supposed to be possessed of treasures, and under the direction of the Danish Consul, and Arago was consequently thrown into slavery. Finally, after a series of vicissitudes of various kinds, he succeeded again in quitting Algiers, and on the 2nd of July, 1809, he entered the lazaretto of Marseilles, with all his instruments which he had succeeded in preserving.

France had believed him dead. The first letter which arrived for him at the Lazaretto was one from Humboldt, who knew him only from his misfortunes, and from that time a friendship commenced between these two great men which continued to the end. On the 17th of the September following, Arago entered the Institute: he was then 23 years old. Already he had made with Biot an extensive work on the determination of the coëfficient for tables of Astronomical refraction; he had measured the refraction of different gases, a research that before had not been attempted; he had determined the relation between the weight of air and that of mercury, and found a specific value for the coëfficient in the formula for calculating the heights of mountains from barometric observations; he had also made an important investigation on the velocity of light, and numerous observations towards the verification of the laws of libration; and finally he completed the triangulation prolonging the meridian of France to the island of Formentosa.

In 1812, the Bureau des Longitudes charged him with the delivery of a course of lectures on Astronomy at the Observatory, which was

continued until 1847, presenting in them the most arduous details of the science. On the 7th of June, 1830, he was named perpetual Secretary of the Academy of Sciences, replacing Fourier. From this moment a new life actuated the Academy, and it was under the impulse received from Arago that this illustrious society attained in a great degree to that distinguished standing and authority now accorded to it by the scientific world.

The revolution of 1830 broke out, and Arago entered political life. Named a member of the Chamber of Deputies, he took his seat among the republicans; and being a great orator, he was not slow to acquire influence in the parliamentary debates. It was on his Report, that a national recompense was voted to Daguerre, the inventor of Photography, and to Vicat, the inventor of hydraulic cements. He voted the printing of the works of La Place and those of Fermat; he defended the railroads against the coalition of the "maitres de porte"; he protected electric telegraphs against the adverse intentions of the administration represented in the Chamber of Deputies by M. Pouillet, the physicist; in a word, in all circumstances, Arago was at the head of Progress.

The revolution of 1848 brought Arago into the Provisional Government. He had just completed his eulogy of Bailey, the Astronomer, the friend of Franklin, who took an active part in the revolution of 1789, of which he was a victim. Reasoning by analogy, Arago looked for a like fate. This fear was happily exaggerated. Times had changed as well as circumstances, and the only analogy between the two men, Bailey and Arago is in that both were astronomers and both perpetual Secretaries of the Academy of Sciences.

After the coup d'etat of December 2, 1851, Arago refused to take the oath of allegiance, which was required of him in his capacity as director of the Observatory, and thus made manifest once more that politics ought to be kept aloof from Science.

A life of so much labor had worn down his health. Although attacked with diabetes, he still contemplated putting the last touch to his unfinished works. Bright's malady set in and aggravated his situation, which was complicated with dropsy of the abdomen, attended with effusions, and swelling of the extremities. All announced his approaching end: yet his mind was not for a moment obscured. Shortly before his death, although blind, he superintended in some difficult researches; he asked M. Babinet to prepare for him a table of more accurately determined numbers for the lengths of undulations, that he might bring to completion a memoir on interferences; and he finished the editing of his Physical researches on the Planets, &c. &c. He died in the midst of these arduous occupations, on the 2nd of October, at the age of 67½ years, a few minutes after having shaken the hand of M. Biot.

We have mentioned some of the works which Arago accomplished in his younger days. These works were completely eclipsed by the discoveries to which his name has since become attached, which embrace the following principles:

1. The discovery of chromatic and rotatory polarization.
2. That of Electro-magnets.

3. That of the magnetism which is developed when bodies are revolved near a magnet.

Arago was an Encyclopedic genius. Science, Literature, Political and Social economy, his vast intelligence embraced all with equal ability. His powerful faculty of assimilation, popularization, and of application of principles, placed him everywhere in the first rank. Whether Orator or Professor, he shone with brilliancy both in political and scientific assemblages. He was distinguished for the perspicuity and elegance of his style, and occupies an eminent place among the prose writers of France.

In the midst of so much grandeur, Arago led a most modest life. He considered as lazy whoever did not work fourteen hours a day; and such days were for him days of repose. Although so absorbed with his occupations, he still found time to appear in the society of Paris as one of its most spirited conversationists.

While devoted to continued labor, he completely forgot his own interests, and had only what was barely necessary for the support of his family. He left two children, one Emanuel Arago, an eloquent orator of the bar of Paris and of Republican assemblies, the other Alfred Arago, a distinguished painter. If he has not bequeathed to them a fortune, he has left an immortal name: he has created by his genius a renown more illustrious than all the renown ever gained by arms—which for a long time enjoyed the privilege of giving fame, but now yields the right to the peaceful conquests of science.

Academy of Sciences.—For some weeks past, there has been little of interest brought before the Academy of Sciences. The visits of several foreign savants, MM. Richard Owen, Magnus, Rammelsberg, Kölliker, etc., have afforded some little variety. But the new scientific communications are few at the present time. I therefore leave this subject to my next letter, when I shall also be able to state who is Perpetual Secretary in place of Arago.

Views on the origin of terrestrial magnetism.—The earliest view of terrestrial magnetism supposed the existence of a magnet at the earth's centre. As this does not accord with the observations on declination, inclination and intensity, Tobias Meyer gave this fictitious magnet an eccentric position, placing it one-seventh part of the earth's radius from the centre. Hansteen imagined that there were two such magnets, different in position and intensity. Ampère set aside these unsatisfactory hypotheses by the view, derived from his discovery, that the earth itself is an electro-magnet, magnetised by an electric current, circulating about it from east to west, perpendicularly to the plane of the magnetic meridian; and that the same currents give direction to the magnetic meridian, and magnetise the ores of iron; the currents, being thermo-electric currents, excited by the action of the sun's heat successively on the different parts of the earth's surface as it revolves towards the east.

A long time before the discovery of electro-magnetism, Biot was occupied with this subject, and regarded the terrestrial magnetism as the principal resultant of all the magnetic particles disseminated in the earth. M. Gauss adopts this view, as an interpretation of the fact,

without explaining it. An observation which I made some years since along with one of my brothers* has directed my attention to this subject. It related to the fall of a cylindrical meteor whose position was sensibly in the plane of the magnetic meridian. Many luminous meteors have been observed in this same position or near it, if I may judge from some of those described in the catalogue of Borguslawsky.†

The special position of the meteor observed by my brother and myself was not fortuitous; it was determined by the magnetic action of the earth, an action which may be powerful in its influence on meteorites consisting essentially of the magnetic metals, iron and nickel. In our view, the terrestrial magnet, the earth, decomposed by influence the normal fluid of the meteoric mass, and so gave the meteor thus polarized the direction of a compass-needle.

In generalising from this fact, and recalling the experiment of Arago on the magnetism developed when a magnet acts upon a turning disc, we ask whether the magnetic polarity of our planet may not be due to a like cause. Considering it, as proved, that the sun is polarized magnetically like the earth,‡ the sun will then be the inductor magnet, the agent which decomposes the magnetic fluid of the terrestrial globe; it will be to the earth, what the earth was to the meteor. This explanation does not resolve the difficulty, as it does not say whence comes the magnetic polarity of the sun. It implies the intervention of a magnet whose intensity is superior to that of the sun, acting on this last by induction, and impressing a polarity which the sun transmits to other planets of the system. It is the hypothesis reversed of the central magnet, for it places in space the magnetic mass which some physicists have supposed to exist within the earth.

The real cause of the magnetic polarity of the planets, is in my view the same for all, and Arago's experiment conducts to it in a straight line. It results even from the condition of their existence. Each star turning around a central axis, and in determinate curves, is influenced by the mass of these stars and their velocity at the circumference; in a word, the agent decomposing into two fluids the normal magnetism of the earth and the other planets, is their rotation. A geometer examining this opinion, would find, we believe, that the declination, inclination and the perturbations of the magnetic needle, are explained on this hypothesis much better than on any other.

Since my researches on circular electro-magnets and in general on bodies in rotation, I have sought much for experimental demonstration of this theory, and have now the conviction that this is impossible, as it is not possible for us while upon the earth to remove ourselves from the action of its own magnetism. Whenever a development of magnetism under the influence of rotation is observed, it is common to attribute it to the inductive action of the earth, rendered so striking by the experiments of Arago and Mr. Barlow.

Alongside of the different sources of magnetism mentioned in Treatises on Physics,—friction, pressure, percussion, torsion,—we should add

* Poggendorff's Annalen, iv, 1.

† See Proceedings, Brit. Assoc., 1853, Sept. 7, Report of Col. Sabine.

‡ Sur la chute d'une bolide par M. N. Nicklès and J. Nicklès, Compt. Rend. de l'Acad., xix, 1035.

rotation, a mechanical action of equal title with the preceding, and whose effects, produced through a subdivision like that of magnetic polarity, are found grouped at the extremities of the axis in rotation; in the same manner as the poles develop at the extremities of a bar of iron when it is subjected to torsion.

Artificial magnets.—For some time, permanent magnets have been made from cast iron by the aid of an electric current. The only difficulty consists in tempering the metal. M. Florimond, Professor of Physics at Louvain, has recently given the results of some investigations on this subject to the Academy of Sciences at Brussels, detailing the effects from using magnets of this kind in the construction of magneto-electric machines, these magnets being much more economical on account of the difference in value of cast iron and steel. The following are some of his conclusions:

1. Gray metal gives more satisfactory results than white metal, which is moreover too brittle.

2. Magnets tempered at a low red heat lose all their magnetism in twenty-four hours.

3. They retain their magnetism perfectly when tempered at a bright red heat.

The following is the method of obtaining the maximum magnetic power. The bars are heated to a red heat in a blast furnace; they are taken out, and powdered over the two faces for $\frac{3}{4}$ths their length with the yellow prussiate of potash pulverized, and then they are plunged immediately into a large quantity of cold water, with violent agitation. When the bars are cooled, they are magnetized by means of a horse-shoe electro-magnet capable of lifting about 200 kilograms. The two poles of the magnet are applied at the place where the branches of the cast iron magnet become parallel; the poles are made to slide quite to the extremities of the branches, and then detached to repeat 3 or 4 times this same process of friction. After operating thus upon one of the faces, the other is subjected to the same treatment, taking care that the same poles are brought into contact with the same branches.

The poles of the bundle of cast iron magnets ought to be always kept in contact with an armature of wrought iron of a size proportional to that of the bundle. The bars of cast iron should be a little thicker than those of steel.

Ascensional force of Balloons in water.—On the 18th of last June, Doctor Gianetti of the mineral springs of Orezza, Corsica, made an experiment with a balloon as a piece of hydrostatic apparatus, before a scientific commission. The object of his balloon was to raise objects from the bottom in deep water; and the force of it is such that with a diameter of 4 metres he was enabled to raise at least 31,000 kilograms. His experiment was made with a balloon of 50 centimeters, which raised 150 kilograms. With hydrogen a much greater effect would be obtained. But Dr. Gianetti, having a practical end in view, uses carbonic acid, which he obtained by the decomposition of a carbonate by means of an acid, at *the bottom of the sea.* He is now proposing to adapt to it a clock movement, which by opening or closing the facet in the top, by which the balloon is filled or emptied, shall cause it to rise or sink at will. It is also proposed to use this invention in river navi-

gation, for the passage of sand banks; the apparatus for this purpose could be secured to the sides of the vessels, and would not add a thickness of more than 2 centimeters (8 tenths of an inch).

Manufacture of Sal-Ammoniac from the residues of gas works.—The Industrial Society of Mulhausen offers annually a number of prizes for inventions and improvements made during the year: and it also offers a prize to those who introduce a new branch of industry into the department of the Haut-Rhin. This last prize was taken by MM. Moerhlin and Stoll, who manufacture sal-ammoniac from the ammoniacal liquid of gas works. The main difficulty in the operation consists in separating the tar-like material which it contains. The following is the process adopted.

The ammoniacal liquid is mixed with slaked lime; then submitted to distillation in a boiler heated by steam; the parts volatilised pass into a worm, in which the larger part of the tar is deposited; the ammonia passes on into a Wolff's apparatus, where it leaves the foreign substances present, and finally is carried into cold water where it is condensed. In this state it is nearly free from its impurities; it is neutralized with chlorohydric acid and evaporated in a lead boiler. As it deposits it is withdrawn by means of a wooden rake; it is allowed to drain, and then introduced into a brick mould and subjected to strong pressure. Blocks of sal-ammoniac are thus obtained, which are dried in an oven heated by part of the heat furnished by the evaporating furnace.

Separation of bromine from iodine.—Balard's process, as carried on by M. H. de Luca, gives a method of recognizing traces of iodine and at the same time of separating it from bromine with which it is so often associated. It is based partly on the greater affinity of bromine for the metals and partly on the violet color which iodine communicates to sulphuret of carbon. An impure bromine is treated by potash (or carbonate of potash which may be more easily obtained pure and free from chlorine); it is evaporated and calcined, to transform the bromate into bromid; it is then neutralized by means of an acid; the liquid is put into a test tube, and a drop of sulphuret of carbon is introduced, after which a drop or two of bromine dissolved in distilled water is added; it is then agitated, and if there is iodine present, the sulphuret of cobalt is colored violet. It is colored yellow by bromine. It is important to avoid an excess of bromine, lest it form a bromid of iodine, which does not act. I have tried the process, and found it exact nearly to a tenth of a milligram of iodine.

Artificial Silicification of limestones.—It is some years since M. Kuhlmann of Lille proposed to preserve pieces of sculpture, etc., by impregnating them with a solution of silicate of potash. $SiO^3\,KO + CO^2\,CaO = SiO^3\,CaO + CO^2\,KO$. This process has been used on a grand scale in certain parts of the cathedral Notre Dame. The architect of the cathedral reports as follows: 1, that the infiltration of silica made "sur les terrasses et contre-fort du choeur," in October, 1852, have preserved the stone from the green moss that covers stones in moist places: 2, that the gutters and flagging of limestone subjected to this process present surfaces perfectly dry, covered with a silicious crust: 3, that upon the stones so prepared, dust and spider webs are

less common than upon the stone in the ordinary state. The report also states that tender stones have been rendered hard; they have lost part of their porosity, and after being washed, they dry more rapidly than stones not silicified. The process has succeeded completely on all calcareous blocks, whether isolated or forming part of the structure, new and old.

It is not yet known how this process will act on mortars; but if successful, the silicification of an entire monument may be accomplished, and its restoration when old. The whole exterior might be thus covered with a thick bed of artificial silicate of lime, and a whole edifice be protected by this means from all atmospheric causes of destruction.

Vitrification of Photographic pictures.—The author of this process, M. Plaut, first procured a photograph on glass covered with albumen, and subjected it gradually to a strong heat so as to redden the glass. The albumen was destroyed, and the photograph, if negative, became positive by reflection. The picture was made of pure silver which adheres quite strongly to the glass, so that it may be polished without alteration.

On exposing this glass to the action of hydrofluoric acid in vapor, an engraving of the design is obtained over parts not covered by the image formed of the silver. It may also be possible to strengthen the image by a galvanic deposit and make a kind of plate from which engravings could be taken.

If, in place of arresting the process at a red heat, it is continued until the glass enters into fusion, the image sinks into the interior of the glass without being altered, and covers itself with a vitreous varnish. It appears like a design of great delicacy, enclosed between two plates of glass; and if positive proofs are employed, the method may be used for making pictured glass which may without doubt be colored by the ordinary processes.

Photographic Portraits on linen cloth.—The Revue Encyclopedique of the Abbé Moigno, from which we have taken the preceding note, states that the problem of making photographs on linen has been resolved. The Abbé Moigno has assisted at the operations of M. Wulff, the inventor; he says nothing of the processes, and we only know that the photographs were taken on linen covered with collodion.

Pyrogallic acid in wood vinegar.—The value of pyrogallic acid in photography gives much interest to the fact brought out by M. Pettenkofer, that it is afforded by the condensing apparatus for purifying gas obtained from the distillation of wood, an invention in which M. Pettenkofer has taken the greater part. M. Pauli is now engaged in the study of this acid. It is without doubt, says Liebig, owing to the presence of pyrogallic acid that we must attribute the preference which dyers give to wood vinegar, an acid which has not been replaced by the ordinary acetate of iron.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the polarization of light by refraction through a metal.*—Biot found that two gold leaves are sufficient to polarize direct solar rays completely. Rollmann has examined the subject anew, and has employed the gold leaves both as a polarizing and as an analyzing arrangement. When the light is very intense, only a single leaf can be employed, as otherwise the field of view appears too dark. When used as an analyzer, a gold leaf shows very distinctly the colors of thin plates of gypsum, cooled glasses, &c., but these are naturally modified by the peculiar blue green color of the gold. If we allow plane polarized light to pass through an inclined gold leaf, and examine by a tourmaline in the light so transmitted a plate of calcspar cut perpendicular to the axis, we shall observe the phenomena of elliptic polarization, when the gold leaf and the analyzer are turned to an angle of 45° with the planes of polarization. The colored rings are narrower in the first and third quadrants than in the second and fourth, the cross is converted into two hyperbolas, whose branches do not meet. When in the above experiment, we leave every thing else unchanged, and examine the calcspar with the analyzer, by means of the light reflected from the gold leaf in place of that transmitted, we observe the complementary figure such as we obtain it when we employ the transmitted light, and the tourmaline is turned through 90°. The tourmaline must be green in order to transmit the light well. Brewster's discovery of the elliptic polarization by metallic reflection is thus extended and completed.—*Pogg. Ann.*, xc, 188.

2. *Additional experiments on the internal dispersion of light.*—In a lecture delivered before the Royal Institution in London, Prof. Stokes has communicated some new observations on internal dispersion, which are of much interest. In accordance with an observation of Faraday, Stokes has found that the blue flame of sulphur burning in oxygen is a source of rays which exhibit the phenomena extremely well. Letters written upon white paper with a solution of chinin, immediately become visible when illuminated with this light, particularly when it has passed through a blue glass, although they are invisible in gas light. The letters remain visible when observed through a glass containing a thin layer of a solution of chromate of potash, but they instantly vanish when this glass is interpolated between the flame and the paper, the solution being impervious to the rays which occasion the color. The author points out in the next place the advantages which prisms and lenses of rock-crystal possess over those of glass, in experiments of this kind, inasmuch as they readily transmit the invisible rays. By employing the light of the powerful galvanic battery of the royal institution, and lenses and prisms of quartz, the author obtained a spectrum six to eight times as long as the ordinary visible spectrum, and crossed from one end to the other with bright bands. The interposition of a plate of glass shortened the spectrum to a small fraction of its original length, the highly refrangible portion being entirely absorbed. The discharge

of a Leyden jar gave a spectrum which was at least as long, but which was not perfectly similar to the others, as it consisted only of insulated bright bands. Stokes remarks finally that in winter, even in bright sunshine, he could obtain no such extended spectrum; as the spring advanced, the light constantly improved; he could not, however, see so far into the spectrum as at the end of the last August. The Earth's atmosphere was evidently not transparent for the very highly refrangible rays of the sun's light.—*Pogg. Ann.*, lxxxix, 627.

3. *On Chemical affinity.*—Bunsen has published an elaborate investigation of the laws of chemical affinity as governing the combination of a body when brought at the same time into contact with two or more other bodies. The views of Berthollet upon this subject are familiar to chemists; Bunsen has however found that Berthollet's law is inaccurate. He substitutes for it as the result of his researches, a new law which may be expressed in the four following propositions.

(1.) When a body A is brought into contact with two or more bodies B, B′, &c., present in excess and under circumstances favorable to combination, the body A, selects only such quantities of the bodies B, B′, &c. as are to each other in a simple stöchiometrical proportion so that together with 1, 2, 3, 4, &c. atoms of the one compound 1, 2, 3, 4, &c. atoms of the other are formed.

(2.) When one equivalent of the compound A+B, is formed in this manner together with one equivalent of the compound A+B′, the quantity of the body B compared with that of the body B′ may be increased up to a certain limit without changing the ratio of the numbers of equivalents. If, however, this limit is passed the ratio of the equivalents suddenly springs from 1 : 1 to 1 : 2, 1 : 3, 2 : 3, &c. The quantity of the substance may now again be increased without changing the ratio of the equivalents until a second limit is attained, when the ratio passes into another, &c.

(3.) When a body A reduces a compound B+C present in excess so that C becomes free a compound of A and B being formed, then if C can exert a reducing return action upon A+B, the final result of the decomposition is such that the reduced position of B+C is in a simple equivalent ratio to the now reduced portion.

(4.) In these reductions also the quantity of one substance may be increased up to a certain limit without changing the atomic ratio. Above this limit, sudden changes of the ratio may occur but always according to small rational numbers.

These laws obviously hold good only when the combinations concerned take place simultaneously, since otherwise the relation of the masses combining is constantly varying. The author consequently studied them in gaseous mixtures, by igniting oxygen with variable quantities of combustible gases.—*Ann. der Chemie und Pharmacie*, lxxxv, 137.

4. *On the supposed new metal, Aridium.*—Some years since Ullgren published a paper upon a substance found by him in a Norwegian chromic iron ore, and which he considered as the oxyd of a new metal closely resembling iron in its chemical properties and relations. Bahr has carefully examined the mineral in question, and finds that the so-called oxyd of Aridium is merely oxyd of iron with a little phosphoric acid and oxyd of chromium.—*Journal für praktische Chemie*, lx, 27.

5. *Preparation of pure Caustic Potash.*—WÖHLER has given a very simple and elegant method of preparing caustic potash in a state of chemical purity. One part of pure saltpetre in powder is to be mixed with from two to three parts of metallic copper cut into small pieces, and the whole heated to a moderate red heat for half an hour in an iron or better still in a copper crucible. After cooling, the mass is to be treated with water, and the resulting lye poured into a narrow cylinder which is then to be carefully closed. After the oxyd of copper has completely settled, the supernatant liquid may be drawn off with a syphon. It contains no traces of copper. The solution is best preserved free from carbonic acid by Mohr's method, namely, by closing the bottle with a cork through which passes air-tight a tube open at both ends, and filled with a coarse mixture of Glauber salt and caustic lime. Iron decomposes saltpetre as completely as copper, but it cannot be employed to prepare pure potash in consequence of its containing carbon, silicon, phosphorus, &c. When the above proportions of copper and saltpetre are used, a portion of the copper is obtained in the form of suboxyd. For a second operation we may take 1 part of nitre, 1 of this oxyd, and 1 of metallic copper. After complete washing, the oxyd of copper may be dissolved in sulphuric acid, and thus converted into blue vitriol.—*Annalen der Chemie und Pharmacie*, lxxxvii, 373.—[This process will be particularly convenient if, as appears probable, the resulting oxyd of copper is in a proper condition to be used in organic analysis. If not, it might be reduced at a low red heat by coal gas, and again employed to decompose nitre.—W. G.]

6. *Density of Selenium.*—SCHAFFGOTCH has determined the density of Selenium, and deduces from a great number of experiments the following conclusions:

(1.) Selenium has two different specific gravities, (at 16° R.) namely, 4.282, and 4·801. The smaller number belongs to an amorphous and glassy condition; the higher one to a granular crystalline state; the two states may be converted into each other at pleasure.

(2.) The blood-red flocky Selenium as precipitated in the cold, has the density of amorphous Selenium, whether its color and apparent volume have been changed by heat or not.

7. *New Alkaloids.*—How has studied the action of the iodids of methyl, ethyl and amyl upon morphine and codein. When finely pulverized, morphine is digested in a closed tube with an alcoholic solution of iodid of ethyl, a white crystalline substance separates, which after recrystallization, is obtained in fine white needles. These are the iodid of ethyl-morphin, the formula of which is $C_{34}H_{18}(C_4H_5)NO_6+HI$. The base was isolated by treating the iodid with oxyd of silver, and appeared as a very caustic liquid of a reddish brown color; which gave no crystals on evaporation, but only a semi-transparent dark colored mass which was deposited from a boiling solution in alcohol as a microscopic crystalline mass. This powder is readily dissolved in muriatic acid to a yellow solution, which gives heavy yellow precipitates with chlorid of platinum and bichromate of potash. The oxyd is readily soluble in water: its probable formula is $C_{34}H_{18}(C_4H_5)NO_6$. With iodid of methyl a similar base was produced, the iodid of which has the formula $C_{34}H_{18}(C_2H_3)NO_6+HI$; the author terms it me-

thyl-morphin. When morphin is heated with chlorid of amyl, fusel oil and chlorid of morphin-ammonium are produced. Codein digested with iodid of ethyl yields a highly crystalline colorless substance which is the iodid of ethyl-codein-ammonium, and which has the formula C_{36} H_{20} (C_4H_5) NO_6+HI. Iodid of methyl yields a similar compound with codein. Methyl-morphin is isomeric with codein, but differs from it in chemical and physical properties.—*Journal für prakt. Chemie*, lix, 489.

8. *Preparation of Valerianic Acid from Fusel Oil.*—Grüneberg recommends the following proportions as the most advantageous. $2\frac{3}{4}$ lbs. of bichromate of potash are to be introduced into a retort, and $4\frac{1}{2}$ lbs. of hot water poured upon the salt. A cooled mixture of 1 lb. of fusel oil and 4 lbs. of sulphuric acid diluted with 2 lbs. of water is to be allowed to flow very slowly and in a thin stream into the liquid in the retort, and the whole is then to be distilled. The distillation goes on quietly, and 9 ounces of oily valerianic acid are obtained.—*Jonrnal für prakt. Chemie*, lx, 169.

9. *Constitution of Butter.*—Heintz has communicated an elaborate paper on the constitution of butter, the results of which are as follows:

(1.) The margaric acid prepared by Bromeis from butter is a mixture of stearic and palmitic acids.

(2.) The fixed fluid acid which is contained among the products of the saponification of butter consists chiefly of common oleic acid, and not as Bromeis believed, of a different acid. There is no butter-oleic acid. Butter therefore contains common olein.

(3.) Among the products of the saponification of butter there is found a fatty acid, the hydrate of which contains more than 38 equivalents of carbon to 4 equivalents of oxygen. This acid, butic acid, has very probably the formula C_{40} H_{40} O_4. It is with great difficulty soluble in cold alcohol, and corresponds to a fat contained in butter which may be called butin.

(4.) Stearic acid is also contained among the products of the saponification of butter, though not in predominating quantity. Butter therefore contains stearin.

(5.) The largest proportion of the solid fatty acids in butter consists of palmitic acid. The largest proportion of the solid fats consists therefore of palmitin.

(6.) Cocinic acid cannot be detected in butter.

(7.) The portion of the solid fatty acids most soluble in alcohol consists of myristic acid. The presence of myristin in butter is therefore to be inferred.

Heintz points out the remarkable fact that in all the acids contained in butter, the number of equivalents of carbon and of hydrogen is divisible by 4. The same law holds good with respect to cocoanut oil. Heintz considers it therefore probable that the cetic and cocinic acids which he detected in small quantity in spermaceti are mixtures, since the numbers of equivalents of carbon which they contain are not divisible by 4 like those of the other acids in spermaceti: he proposes to resume the subject, operating upon 10 lbs. of spermaceti.—*Pogg. Ann.*, xc, 137.

10. *Preparation of Ferrocyanhydric Acid.*—LIEBIG gives the following simple method of preparing this acid. When a cold saturated solution of ferrocyanate of potash is mixed with its own volume of fuming muriatic acid added in small portions at a time, a snow-white precipitate of pure ferrocyanhydric acid is thrown down. These are to be washed with muriatic acid, dried upon a brick, and dissolved in alcohol; from the alcoholic solution the acid may be obtained in beautiful crystals.—*Ann. der Chemie und Pharmacie*, lxxxvii, 127.

11. *Separation of Nickel from Cobalt.*—LIEBIG has found that when a current of chlorine is passed into a cold solution of the double cyanides of cobalt and potassium and of nickel and potassium, the liquid being kept alkaline by the addition of caustic soda or potash, the nickel is completely converted into sesquioxyd and precipitated, while the cobalt remains in solution as unaltered double cyanid. The sesquioxyd of nickel may be washed and ignited, and the nickel weighed in the form of protoxyd; it is perfectly free from cobalt. The solution after passing the chlorine must still be alkaline. The smallest trace of nickel gives an inky black color when dissolved in cyanid of potassium, and treated with chlorine. This method of separating cobalt and nickel has perhaps some advantages over Liebig's second method which, it will be remembered, consists in boiling the mixed double cyanids with oxyd of mercury, which precipitates the nickel but not the cobalt.

12. *On a general method of volumetric analysis.*—BUNSEN has given a very accurate and elegant method for the volumetric determination of a great number of substances. The principle of the method consists in bringing the substance to be determined into such a state of oxydation that when boiled with muriatic acid chlorine shall be evolved. The chlorine is to be conducted into a solution of iodid of potassium, and the evolved iodine volumetrically determined by means of a solution of sulphurous acid. In those cases in which a substance is susceptible of two degrees of oxydation, neither of which evolves chlorine with muriatic acid, the author boils with muriatic acid and a known weight of bichromate of potash; a portion of the chlorine goes to peroxydize the oxyd analyzed, while the other portion passes off and sets free an equivalent quantity of iodine. The author gives a number of examples of the application of the method, in which results of surprising accuracy were obtained: for the details of the method, which would carry us beyond our limits, we must refer to the original memoir, which can hardly fail to mark the commencement of a new era in analytical chemistry. In the original memoir the author points out the advantages of the method in the determination of chlorine, bromine, iodine, sulphurous acid, sulphydric acid, the chlorates, chlorites, and hypochlorites, chromates and iodates, the superoxyds of lead, manganese, nickel, cobalt, &c., and finally the oxyds of iron and of cerium, and arsenious acid. To show the accuracy of the method, we give the results of two analyses, the first of a magnetic iron from the Tyrol, the second of a mixture of white arsenic and gypsum. The ore contained:

	By calculation.	By analysis.
Fe_2O_3 . .	68·97	68·96
FeO	31·03	31·04
	100·00	100·00

The mixture contained AsO_3 33·15, and CaO, SO_3 66·85; the analysis gave AsO_3 33·14, and CaO, SO_3 (by difference) 66·86.—*Ann. der Chemie und Pharmacie*, lxxxvi, 265.

13. *Volumetric determination of manganese.*—KRÜGER has studied in Bunsen's laboratory the application of the volumetric method to the determination of manganese more especially when combined with other oxyds. The method of determining the quantity of manganese in the common deutoxyd will serve as an example of the process employed. A few decigrammes of the oxyd are introduced into a small flask which is then filled two-thirds full of pure concentrated muriatic acid. The chlorine evolved on heating is conveyed by a glass tube attached to the flask by vulcanized rubber into a solution of iodid of potassium. An equivalent of chlorine sets free an equivalent of iodine which remains dissolved in the excess of iodid of potassium and colors the solution brown. After the evolution of chlorine has ceased, a measured portion of the normal solution of sulphurous acid is added till the brown color has vanished. The excess of sulphurous acid added is then determined by means of a titrated solution of iodine in iodid of potassium, a few drops of a clear solution of starch having been added. The quantity of this solution required to oxydize one measure of the normal solution of sulphurous acid having been determined, the quantity of manganese may be calculated. It is well known that the oxyds of manganese are all converted by ignition into Mn_3O_4. In the presence of strong bases, however, this is not always the case, and a special investigation of this point was necessary. Krüger obtained the following results. To determine manganese in the presence of iron, the iron is to be peroxydized and the two oxyds precipitated together by carbonate of soda: the precipitate is to be well washed, dried and ignited, and then the manganese determined volumetrically. It is present as Mn_3O_4. The determination of manganese in presence of alumina and glucina is precisely similar; in both cases the oxyd after ingnition is Mn_3O_4. When, however, the mixed carbonates of manganese and copper, lead, cadmium, bismuth, zinc, magnesia, baryton, stratia or lime are ignited, the manganese is always present in the form of sesquioxyd and this must be borne in mind in calculating the result of the volumetric determination. The author further remarks that in order to determine manganese when only iron or alumina are present it is sufficient to ignite the compound with free access of air and then submit a weighed portion of the ignited compound to the volumetric determination calculating the manganese as Mn_3O_4. When one or more of the other oxyds are present in quantity sufficient to form a manganite, (Mn_2O_3 as manganous acid,) the same process may be empolyed, but the manganese must be calculated as Mn_2O_3. When the quantity of the other base is unknown or insufficient to form a manganite, a weighed quantity of the compound may be dissolved in muriatic acid and half its weight of oxyd of zinc added. The mixed oxyds are then to be precipitated with carbonate of soda, and the precipitate washed, dried and weighed. The manganese in a weighed portion of this precipitate may then be determined volumetrically: it is present as Mn_2O_3. The results obtained by the method in question are remarkably accurate.—*Ann. der Chemie und Pharmacie*, lxxxvii, 257.

14. *Vessels for the preservation of fluohydric acid.*—STÄDELER has found that gutta percha and vulcanized India rubber resist the action of fluohydric acid almost completely. A solution of the acid which was so concentrated as to fume in the air, was found, after having been for some time preserved in a bottle made of gutta percha, perfectly colorless and clear. The gutta percha had undergone no change, but was somewhat brighter colored on the inside.—*Ann. der Chemie und Pharmacie*, lxxxvii, 137.—[It would doubtless be possible to cover the inside surface of a glass bottle with gutta percha by pouring in a solution of the gum resin in chloroform.—W. G.] W. G.

II. MINERALOGY AND GEOLOGY.

1. *Parophite.*—A rock allied to the Dysyntribite of Shepard, and Rensselaerite of Emmons, has been named Parophite by T. S. Hunt, (Logan's Rep. Geol. Survey of Canada, Quebec, 1852, p. 95.) The name alludes to its resemblance to Serpentine, notwithstanding its non-magnesian character. It occurs imperfectly schistose, as well as massive, and sometimes a perfect schist; rarely botryoidal, with an appearance of concentric structure. Texture granular to compact. G. 2·7–2·784. Color pale greenish, yellowish green, olive green, ash grey, reddish. Lustre waxy, shining; subtranslucent. Hardness not over 2·5–3; cuts with a knife like massive talc. Composition, according to Mr. Hunt—

	Si	Al	Fe	Ca	Mg	K	Na	H	
1. Schistose,	48·50	27·50	5·67	1·30	2·24	5·30	1·91	7·00	G.=2·705
2. Schistose,	48·42	27·60	4·50	2·80	1·80	5·02	2·78	6·88=99·80	G.=2·714
3. Botryoidal,	49·13	27·80	5·90	3·80	1·40	*undet.*		6·30	G.=2·784
4. Schist,	48·10	28·70	4·80	2·10	1·41	4·49	1·53	8·40=99·53	

Crystalline limestones of the Vosges.—DELESSE describes with much detail this rock, (Bull. Soc. Geol. France, ix, 120.) He mentions the following minerals as found in the limestone which occurs in gneiss: *Phlogopite*, *pyrosclerite*, *green and gray pyroxene*, *brown hornblende*, *graphite*, *spinel*, *chondrodite*, *magnetic pyrites*, *iron pyrites*, *orthoclase*, *a triclinic feldspar*, *sphene*, *quartz*, *tremolite*, *chlorite*, etc. There are frequent concentric nodules in the rock, which consist, beginning at the centre, of feldspar, pyrosclerite, mica. Amphibole, pyroxene, sphene are common in the feldspar or pyrosclerite of these nodules. Delesse shows that the pyrosclerite has resulted from the alteration of the feldspar nodule previously formed. This paper enters into important discussions respecting the origin of crystalline limestones and their minerals.

Pyromeride of the Vosges; M. DELESSE, (Bull. Geol. Soc. France, [2], ix, 175.)—This Pyromeride is a kind of quartziferous porphyry, containing small globules through it. The variety from the Vosges closely resembles that from Corsica. Delesse finds that the globules consist mostly of silica, being a mixture of silica (over 60 per cent.) and feldspar. He obtained in an analysis :—

Si	Al	Fe	Ca	Mg	K, Na (by diff.)	H
88·09	6·03	0·58	0·28	1·65	2·53	0·48=100

The origin of the globules is attributed to the tendency of feldspar to crystallize, and the indirect action of the excess of silica present. There is an analogy between them and the so-called perlites and retinites. Feldspar never takes the globular form, except in rocks rich in silica. The rock contains specular iron. Delesse concludes that the silica was introduced, subsequent to the first origin of the rock. It occurs either in veins or intimately mixed with the rock material. Other rocks may become pyromerides through a penetration with silica.

Pitchstone from the trap of Isle Royal.—The following analysis is by Foster and Whitney (Rep. Lake Superior, Part ii.):

$\ddot{S}i$	$\ddot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{N}a$, *trace of* $\dot{K}$	$\dot{H}$
62·51	11·47	11·05	2·67	2·11	3·03 (*loss*)	7·14

B.B in the forceps, swells up, becomes nearly white, and then fuses without much difficulty to a grayish glass. Imperfectly attacked by hydrochloric acid when pulverised.

Oxyd of Zinc.—According to G. Rose, (Kryst. Chem. Min., 1852, p. 64,) artificial crystals of oxyd of zinc, a furnace product, sometimes present a six-sided pyramid terminating the hexagonal prism, the former replacing the basal edges of the latter: and the angle of the pyramid is 127° 40′, very near the corresponding angle in corundum, which is 128° 3′—thus showing an approximate isomorphism between a protoxyd and a sesquioxyd.

Crystallized Furnace Products.—F. Sandberger has announced the occurrence as furnace products, of graphite in 6-sided tables near Dillenburg; metallic copper in threads and rarely octahedral crystals, near Dillenburg; antimonial nickel in long hexagonal needles, at Ems; galena in cleavable cubes, at Holzappel and Ems; magnetic iron in octadra; $3Cu^2\,O+SbO^3$ in copper red or yellow hexagonal tables, at Dillenburg; $Ti\,Cy+3Ti^3N$ in Bodenstein.

Fibrous amianthoid substance, a furnace product from Westphalia.—Schnabel obtained for this substance,

$\ddot{S}i$ 98·13, $\ddot{A}l$ 1·24, $\dot{C}a$ 0·46, $\dot{M}g$ and $\dot{F}e$ *a trace*, =99·83

G.=2·59.—(Pogg. Ann., lxxxv, 462.)

Dolomite.—M. J. Durocher has obtained Dolomite artificially through the action of magnesia vapors. He put in a gun barrel some anhydrous chlorid of magnesium and a porous carbonate of lime, the latter being so placed that it could be reached only by vapors from the former. The gun barrel was closed and then kept at a low red heat for three hours. The limestone when taken out, was partly scoriaceous externally, and covered with a mixture of chlorid of calcium and chlorid of magnesium. Within, it was altered mostly to a dolomite, as ascertained by analysis.

Pseudomorphous Minerals:—

Pinite after Labradorite.—Resembles a yellowish gray talc-like mica. H.=2·5, G.=2·832. Analysis by A. Knop and W. Knop, (Pharm. Centr. 1852, 165; Lieb. u. Kopp's Jahresb. f. 1851, 822):

$\ddot{S}i$	$\ddot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{N}a$	Fl	$\dot{H}$
55·18	27·51	4·08	0·29	1·22	3·36	4·49	0·07	3·74=99·94

Formula deduced, $\dot{R}^3\,\ddot{S}i^4+5\ddot{A}l\,\ddot{S}i+3\dot{H}$.

Karpholite after Wolfram.—R. Blum, (Pogg. Ann., lxxxiv, 154.)

On Serpentine after Hornblende, Augite, Diallage, Schillerspar; by G. Rose, (Pogg. Ann., lxxxii, 511.)

On White lead ore after Linarite; by W. Haidinger, (Jahresb. der k. k. oestr. geol. Reich., 1851, ii, 78, and Lieb. u. Kopp, Jahresb. f. 1851, p. 824.)

On the waters of the Great Salt Lake, Rocky Mountains; by Dr. L. D. Gale, (Stansbury's Expedition to the Great Salt Lake, Philadelphia, 1852).—Amount of solid contents, 22·422 per cent. Specific gravity, 1·170. Composition:

Chlorid of sodium, ...	20·196
Sulphate of soda, ...	1·834
Chlorid of magnesium, ...	0·252
Chlorid of calcium, ...	*trace.*

On the Waters of the Warm and Hot Springs of Salt Lake City; by Dr. L. D. Gale, (ibid.) The mineral water of the warm spring has a strong smell of sulphuretted hydrogen. Specific gravity 1·0112. Solid matter afforded on evaporation 1·08200 p. c. Analysis afforded,

Sulphuretted hydrogen uncombined, .	0·037454
" " combined, .	0·000728
Carbonate of lime precipitated by boiling,	0·075000
" magnesia " " .	0·022770
Chlorid of calcium, ...	0·005700
Sulphate of soda, ...	0·064835
Chlorid of Sodium, ...	0·816600
	1·023087

The Hot Spring has the specific gravity 1·0130, and yielded 1·1454 per cent. solid contents. Composition in 100 parts:

Chlorid of sodium,	0·8052,	Chlorid of magnesium,	0·0288,
Chlorid of calcium,	0·1096,	Sulphate of lime,	0·0806,
Carbonate of lime,	0·0180,	Silica, . .	0·0180=1·0602

Analyses of several native Borates: by Prof. Bechi, (from a letter from Prof. Meneghini to J. D. Dana, dated Pisa, July 26, 1853.)—The borates examined by Prof. Bechi occurs as incrustations at the baths of the Lagoons of Tuscany.

(1.) *Lagonite.* Analysis:

$\dot{B}$ 47·955 $\dot{F}e$ 36·260 $\dot{H}$ 14·016 $\dot{S}i$, $\dot{M}g$, $\dot{C}a$, *and loss,* 1·769=100

Formula hence deduced, $\dot{F}e\,\dot{B}^3+3\dot{H}$.

(2.) *Hayesine?* Analysis:

$\dot{B}$ 51·135 $\dot{C}a$ 20·850 $\dot{H}$ 26·250 $\dot{S}i$, $\dot{A}l$, $\dot{M}g$ 1·750

leading to the formula $\dot{C}a\,\dot{B}^2+4\dot{H}$.

(3.) *Borax?* Analysis:

$\dot{B}$ 43·559 $\dot{N}a$ 19·254 $\dot{H}$ 37·187=100

whence the formula, $\dot{N}a\,\dot{B}^2+6\dot{H}$.

(4.) *Larderellite,* (new species.)—White and very light, tasteless, appearing under the microscope to be made up of minute oblique rect-

angular tables; M : T = 110°, according to a measurement by M. Amici. Analysis:

$\dot{B}$ 68·556 NH^4O 12·734 $\dot{H}$ 18·325

The formula deduced is $NH^4OB^4+4\dot{H}$. It dissolves in hot water, and is transformed into a new crystallized salt, which is represented by the formula $NH^4O\dot{B}^6+9\dot{H}$.

On Melan-Asphalt; by C. M. WETHERILL, (Trans. Amer. Phil. Soc., x, 353.)—This mineral is the same that has been pronounced bituminous coal by other investigators. It is from the Albert coal mine, New Brunswick. Some of the reasons for considering it coal are cited in this Journal, vol. xiii, 277. Dr. Wetherill states that E. Durand of Philadelphia, obtained for the solubility of the asphaltum of Cuba 34 parts in ether, and 60 parts in oil of turpentine, with 6 residue; and of the Hillsborough material, 4 parts in ether, and 30 in turpentine, with 36 of residue. Cannel coal gives no solution with turpentine.

Analyses, afforded—

	Carbon.	H	O, N
1. Asphaltum of Cuba,	82·670	9·141	8·189=100
1. Melan-asphalt,	86·123	9·871	4·906=100

Dr. Wetherill calculates the formula $C^{68}H^{42}ON$, from the latter analysis. The Hillsborough product is stated to be unlike coal in becoming electric by friction. [It may be questioned whether this substance can be considered a simple chemical compound. It is more probable from the trials with solvents and other tests that there is a very large excess of carbon, as impurity.]

2. *Thalia.*—The following letter from Dr. Genth to Dr. D. D. Owen, respecting his new earth Thalia, written, as he informs us, in June last, is from the Proceedings of the Academy of Natural Sciences of Philadelphia. Dr. Genth writes us that his investigations were independent of those of Professor J. Lawrence Smith.

"I have just completed the experiments with your thalia, and have come to the conclusion that it is nothing but magnesia. Magnesia shows sometimes such a strange behavior with reagents, that one is inclined to think it a new earth. I had the same case with my analyses of Kämmerite (Rhodophyllite.) It is possible that the relations which exist in the mineral had not been destroyed, and that you have a solution of the mineral,—for instance, a solution of aluminate of magnesia. I separated both with acetate of potash, and free acetic acid and carbonate of baryta. The only strange reaction was, that it fell down with $NH_4O, \dot{C}$ in presence of NH_4Cl, but I find it now in all the magnesian minerals which I examine in a similar manner.

From the oxalate of your thalia I prepared the pure earth. With cobalt solution before the blowpipe it gave a flesh colored mass. Dissolved readily in very dilute acid, and gave no precipitate with ammonia in presence of chlorid of ammonium, and all the reactions of magnesia. The sulphate gave with sulphate of ammonia the well known double salt in oblique rhombic prisms. The pure sulphate with seven equivalents of water crystallized right rhombic, and had the form, appearance, taste, and gave all the reactions of epsom salt. It gave me 50·8 per cent. of water, 35·5 per cent. of sulphuric acid, which also proves that I

had sulphate of magnesia. The analysis of the mineral is, according to Dr. J. L. Smith, and also the researches in my laboratory:

	Ḣ	S̈i	Ä̇l	Ḟe	Ċa	Ṁg	K̇
J. L. Smith,	20·66	45·66	4·87	2·09	3·07	22·10	0·15=98·45
E. Reakirt,	19·96	44·07	4·72	1·70*	3·75	21·49	not det.
			7·79		26·60		K̇ 0·12 Ṅa 0·16
P. Keyser,		44·66					

According to these analyses the mineral is Saponite."

3. *A new Meteorite from Tennessee;* by Prof. J. Lawrence Smith, (from a letter to J. D. Dana.)—The meteoric iron was found in East Tennessee a short while ago, and weighed originally over 60 lbs. It is a highly interesting one, and has furnished for the first time the solid protochlorid of iron, found in a fissure. It is also rich in the phosphuret of iron and nickel, and furnishes material for a full investigation of this latter mineral. The examination is nearly complete, and when finished, a full history of the meteorite will be given.

4. *On the Identity of Owenite and Thuringite;* by Prof. J. Lawrence Smith, (from a letter to J. D. Dana.)—The perfect correspondence of the compositions of Owenite and Thuringite, except in the alumina, led me to an examination of both these minerals. The analysis of Owenite accords with Dr. Genth's; that of Thuringite shows, that in the former analysis of it, sixteen per ct. of alumina has been overlooked. Full details of these particulars will appear in the fourth part of the reëxamination of American minerals.

5. *On the probable depth of the Ocean of the European Chalk Deposits;* by Prof. H. D. Rogers, (Proc. Bost. Soc. Nat. Hist., 1853, 297.)—Various geologists, and among them Prof. Ed. Forbes, in his excellent and learned Palæontology of the British Isles in Johnston's Physical Atlas, have suggested that the Ocean of the Chalk deposits of Europe was a deep one; and in evidence of this, Prof. Forbes cites the "striking relationship existing to deep-sea forms of the English Chalk Corals and Brachiopods, adding that the peculiar Echinoderms, (Holaster, Galerites, Ananchytes, Cidaris, Brissus, and Goniaster) favor this notion, as also the presence of numerous Foraminifera.

I beg leave to present a difficulty in the way of this conclusion. Several of these genera of Echinoderms, as *Ananchytes*, *Cidaris*, *&c.* occur in the Green Sand deposit of New Jersey, referable by every fossil test to the age of the Green Sand and Chalk of Europe. And this American stratum was unquestionably the sediment of quite shallow littoral waters. That they must have had a trivial depth is proved by the circumstance that they repose in almost horizontal stratification, at a level of not more than from one hundred to two hundred feet lower than the general surface of the hills and upland region to the N. W. of the margin of the zone they occupy as their outcrop. It is obvious that a depression of the cretaceous region, such as would cover the present deposits with a deep sea, would have likewise overspread the low Gneissic hills to the N. W. of the Delaware, which present no traces of having ever been submerged during the cretaceous or any secondary period.

* The sesquioxyd of iron and alumina contain a trace of silica, which was not separated.

Mr. AYRES remarked, that of those genera of Echinoderms, which Mr. Forbes regarded as deep sea genera, two or three are found in North America in water not two hundred feet deep. Terebratula, which has been generally regarded as only an inhabitant of very deep water, and whose structure has been described as admirably adapted to the depth at which it has been found, and which Prof. Owen has demonstrated cannot exist at a depth of less than two or three hundred fathoms, exists at Eastport, Me., in water so shallow that it can be taken by hand. In the same locality and position, Radiata are found which have heretofore been thought to be only inhabitants of deep water. Some of Mr. Forbes's genera are also found in less than ten fathoms of water.

III. BOTANY AND ZOOLOGY.

1. *Salad for the Solitary; by an Epicure.* New York: Lamport, Blakeman & Law.—A book which has a very large sale, and is commended as a model of erudition. A chapter in it, which treats of curious matters concerning plants, having casually attracted our attention, we here refer to it, to illustrate, *negatively*, the proposition, that to moralize edifyingly upon any department of nature, or to draw felicitous illustrations, requires that an author should know somewhat of the subject matter he writes about. The following specimens are culled from a dozen pages of the book (from p. 185 to 197). The Papyrus of Egypt is said to be a tree! Of the tree which furnishes the Peruvian Bark, "its trunk, owing to the *frequent scaling of the bark*, is said to be seldom seen thicker than the arm, although it attains a great height." Camphor is said to be distilled "from the *roots* of a tree, growing in *Borneo and Sumatra*." "It is the leaves of plants and trees that act upon the air *like* human lungs, by absorbing carbon and evolving vital air for animal respiration,"—a curious and confusing way of expressing what is meant. The Cypress tree at Chapultepec, near the city of Mexico, our author affirms to be "one hundred and seventeen feet ten inches in girth." The two latest measurements that we have heard of made it 41 or 45 feet: but this was several years ago! On reading further, we perceive that the author has mixed up various famous Cypresses of Mexico into one *salad;* and has then applied De Candolle's (or as he writes De Candalle's) remark on the tree of Santa Maria del Tule to that of Chapultepec. Contrary to the best-grounded opinions, we are told that the Great Chestnut of Mount Etna was probably formed of a single trunk. Of "the colossal Water-lily of British Guiana," doubtless the Victoria, it is gravely said that "its flower measures from four to five feet in circumference." Our Magnolia grandiflora is said to be a "tropical plant;" with "leaves from eight to nine feet in length: its beautiful white blossoms are of like dimensions." Truly of such a tree he would have reason to say: "it is doubtless one of the most superb vegetable productions of which we have any knowledge." If we reduce these feet to as many inches, the statement would even then be somewhat exaggerated. The Tulip-tree, which surely the author might be personally familiar with, is said to have "*brilliant* glossy leaves, and blossoms, giving their odor to the

stars, and despising the minor denizens of the forest." To the stars, indeed, they must give their odor; for they have none for man. Discoursing of flowerless vegetation, our author states, that "even yeast .... is supposed by botanists to belong to this *genera* of the vegetable world;"—so that, after all, the botany is as good as the grammar. As a piece of vegetable morphology, we are told that "seeds are merely leaves preserved in peculiar ceremonies;" and in respect to these coverings a series of statements follows, the logical connexion of which may perhaps be divined. If successful, the reader may next attempt to extricate the author's meaning from the confused statements respecting the boundary between the vegetable and the animal kingdoms. Lastly, the Venus' Fly-trap is called "a native of Canada, which, not unlike other natives of that soil, discovers singular irritability of temperament." Unfortunately for the hit, such as it is, the Fly-trap is exclusively found in North Carolina. Whatever the rest of the book may be, surely, as to this chapter, the intelligent reader will hardly be able to appropriate one of the mottoes of the volume, taken from old Quarles.

"The *herbal* savor gave his sense delight."

Let us open the book in another place. On p. 92, we read that "paper is produced from a beautiful fibrous plant, called Linum, or flax, the *leaf* of which is rotted, and passing through certain processes, becomes *cotton cloth*," &c. Truly, ignorance, however preposterous, is not necessarily a sin *per se*. Its heinousness depends very much on the use that is made of it. A. G.

2. *Lindley: The Vegetable Kingdom; or the Structure, Classification and Uses of Plants, illustrated upon the Natural System; with upwards of* 500 *illustrations.* Third edition: with corrections, and additional genera. London: Bradbury & Evans, 1853. Roy. 8vo.—By intercalation (the work being stereotyped) and otherwise, this invaluable book is extended to about a thousand pages; and the latest additions to the science down to the time of publication are incorporated. It is an indispensable work, especially to teachers and travelling botanists. No other single volume contains a tithe of the valuable information that is condensed into this. The author's untiring industry and vigilance are shown in the accumulation and careful elaboration of this vast amount of materials. His signal ability and acumen are equally displayed in the copious critical matter, often of great importance, and in the defence of his peculiar views of the classification and affinities of plants; many of which, nevertheless, have been, and may be, we think, successfully combated. A. G.

3. *De Candolle's Prodromus.*—Our Botanists may be glad to know that the printing of the fourteenth volume of this work (to contain the *Polygonaceæ*, *Thymelaceæ*, *Proteaceæ*, &c.) has at length commenced. Probably the volume cannot be published before the early spring. A. G.

4. *Observations on the habits of certain Crawfishes*, (in a letter of Dr. R. P. Stevens to the Smithsonian Institution.)—While examining a coal mine, along the banks of Coal Creek, a tributary of Green River, Bureau Co., Ill., I found innumerable little paths of an *Astacus*

leading from the water, along the sands and up into the neighboring low lands. Visiting these paths early in the morning, while yet the fogs were unrisen, I often found the *Astacus* returning from his meadow rambles, but never could ascertain the precise object of these rambles, whether predatory or otherwise.

A very interesting fact in relation to the habits of that animal I had an occasion to witness on the prairie near Chicago. On the ridge dividing the waters of the Chicago River and the Aux Plaines, is a wet marsh, lying near the deposit of bituminous limestone. The marsh, at the time of my crossing it, was quite dry. In search of fresh water shells, my attention was soon directed towards a large number of the paths of the *Astacus*, some of them showing evidences of a very recent traveller. I had not met with any water since crossing the Chicago River, two miles distant. Curiosity led me to follow up one of the freshest trails, until I discovered the retreat of the Crawfish. In my examination I crossed and recrossed the marsh in order to ascertain whether there was anywhere some little pool of water, but finding none I was soon delighted to find an *Astacus* leisurely travelling along his own highway, bearing in his mandibles a coleopterous insect. I followed on his trail to know what would become of him, when suddenly he was lost. By careful examination in the rank herbage, I found he had disappeared in a well or cistern about 10 or 12 inches deep and 1½ wide. Here was his own pool, provided for by himself, for the long summer draughts. In following up other trails I invariably found them terminating in similar pools. In many the inmate was present, whilst absent in others."

Extract from a second letter from Dr. Stevens, dated Nov. 23.—

"Our friends, the *Astaci*, increase in interest as I become more and more acquainted with their habits and instincts. I have learned this month that they are migratory, and in their travels are capable of doing much damage to dams and embankments. On the little Genesee, they have within a few years compelled the owners of a dam to rebuild it. The former dam was built after the manner of dykes, i. e. with upright posts, supporting sleepers laid inclining at an angle of 45° up the stream. On these were laid planks, and the planks covered with dirt. The *Astacus* proceeding up stream, would burrow under the planks where they rested on the bottom of the stream, removing bushels of dirt and gravel in the course of a night. I have seen this season, where they had attempted the present dam, piles of dirt, of at least one bushel.

They now travel over the dam in their migration, often climbing upright posts two or three feet high, to gain the pond above."

It is to be regretted that no specimens were procured in order to ascertain to which species the above Crawfishes belong: whether to *Cambarus fossor*, or *C. diogenes*, or some other. This deficiency, however, Dr. Stevens promises to supply hereafter.

No mention is made by Dr. Stevens of any mud chimneys being built upon the exterior surface of the wells, such as are constructed by *C. diogenes* in the district of Columbia. S. F. Baird.

IV. Miscellaneous Intelligence.

1. *On the Earthquake at Manilla, of Sept.* 16, 1852, (Proc. Bost. Soc. Nat. Hist., 1852, 300.)—The first shock occurred at 7 o'clock on the evening of September 16, 1852. The inhabitants all ran into the streets, expecting every moment the houses to fall into ruins, foreigners looking on with awe and astonishment, and the natives, better aware of the danger, were on their knees devoutly praying. The houses are built of stone, with very thick walls, and rather low, in order to withstand better such shocks of earthquakes, and yet many of them were completely destroyed. In one of the strongest houses, an occupant writes, that the lower story did not move much, but the upper one swayed to and fro, to use his expression, "like a blade of grass in the wind." The noise made by the breaking of walls, the falling of furniture, and the cracking and creaking of the timbers was such as to impress every one with an exaggerated idea of the destruction of property. The shock lasted about one and a half minutes; during the evening there were four more distinct shocks, at regular intervals of about an hour, namely, at eight, nine, ten, and eleven, and another at four the next morning. At each shock the great bell of the cathedral tolled, followed by all the bells of the city.

At night the city was almost deserted, from the danger of remaining in houses with tiled roofs; the inhabitants fled to the native houses of the suburbs with thatched roofs, and many slept in boats on the river. For two or three days after, there were several slight shocks, and for weeks several ships in the river were used as lodging houses.

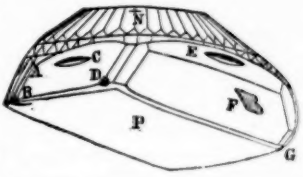
This was the longest and most severe earthquake that has visited these islands for two hundred years. The damage to property was considerable, though the loss of life was small; only three or four lives are known to have been lost. Almost every stone house suffered more or less, according to its strength; nearly all the government barracks, the custom house, colleges, palace, theatre, and many private dwellings were rendered completely untenantable. Two churches were destroyed. One, the oldest in Manilla, founded nearly three hundred years ago by the Jesuits, very large, with walls and arches four feet thick, was thrown down into one immense mass of ruins. The movement was not slow and gradual, like a long heavy swell, but a quick succession of short sudden shocks. The effects of the shocks were different in different parts of the island; there did not seem to be any regular track pursued by the earthquake; in places within a few miles of each other, in one it was not felt at all, while in the other it was quite severe. At Mariveles, just across the Bay from Manilla, the earth opened with an eruption of black sand, which covered the country for a considerable extent; how large the opening was at the time is not known, but it is now seven hundred yards long and one yard wide. The volcanoes at Albay and Taal, which have not been in operation for many years, have been since discharging lava, stones, &c., with considerable activity."

Observations by Prof. H. D. Rogers.—Prof. Rogers referred to the circumstance that the undulatory movement of an earthquake is felt

much more sensibly at a point above the earth's surface than directly upon it. An instance illustrating this had come within his own knowledge. The earthquake which destroyed the principal city of Guadaloupe was felt in the city of New York, but only in the fourth story of a printing office. The sound generally precedes the shock, as has been observed in this country. In North America, the undulation is always parallel to the physical features of the continent, making it reasonable to believe that through a long series of epochs the motion has been in one rather than various directions, as supposed by Elie de Beaumont. There are two movements in earthquakes; an undulating and a molecular movement. The latter, Prof. Rogers thought was the movement which attracted most observation, giving rise as it does to sudden and abrupt changes of relation on the surface of the earth at places where the formation of the strata admits of more or less freedom of movement, causing the sudden shocks which are so destructive. Prof. Rogers gave an account of some of the opinions of geologists as to the thickness of the earth's crust. He gave it as his own opinion, that in most places it is not more than ten miles thick.

2. *The Koh-i-noor Diamond*, (from a Lecture by Prof. TENNANT.)—On closely examining the Koh-i-noor Diamond at Buckingham Palace, in company with my friend, the Rev. W. Mitchell, I found that two of the larger faces were cleavage planes; one of them had not been polished, or it had been so slightly polished that the effect was not discoverable. By measuring the stone with a goniometer, and ascertaining the inclination of its larger faces, 109° 28′, I ascertained which were the cleavage planes and which the cut planes of the diamond. Upon further examination I found two other cleavages, which make the stone correspond with an octahedron. * *

The flaws in the Koh-i-noor are shown in the annexed figure.



P, is a large plane at the base of the diamond, which is a cleavage plane. F, also a large cleavage plane, produced by a fracture—this had not been polished—and being inclined to the plane P at an angle of 109° 28′, it afforded a satisfactory means for determining the direction of the cleavage planes of the stone.

A shows a flaw running parallel to the cleavage plane F: this constituted the principal danger to be apprehended in cutting the stone, and was most skillfully ground nearly out before any of the facets were cut. This flaw seemed to proceed from a fracture marked B.

C and E were little notches cut into the stone for the purpose of holding the diamond in its original setting.

N, a small flaw, which almost required a glass to see it, evidently parallel to the plane P.

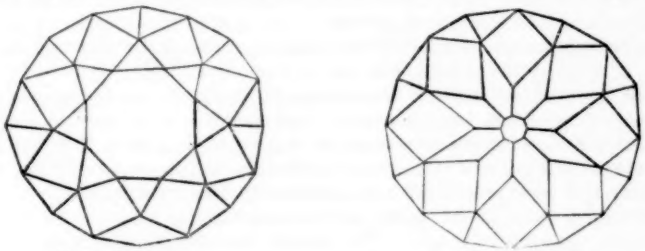
D, a fracture from a blow or fall, showing at its base a cleavage plane.

There is every probability that the Koh-i-noor is only a portion of the original diamond of that name, as procured from the mines of Golconda. My own opinion is, that in its original form this diamond was

a rhombic dodecahedron, and that in its present state it is about one-third of the original size. I am confirmed in this opinion by Tavernier, who states that it originally weighed 787½ carats; after having been broken or cut, it weighed 279-9/16; and if we make allowance for the difference between the French and English grains of that period, it would reduce it to 252 carats of the present time.* If we give Tavernier credit as to the original weight of the stone, we may indulge in a very reasonable supposition that two other remarkable existing diamonds once formed part of it. Dr. Beke, in a paper read before the British Association at Ipswich, in 1851, says, "At the capture of Coochan, there was found among the jewels of the harem of Reeza Kooli Khan, the chief of that place, a large diamond slab, supposed to have been cut [*broken ?*] from one side of the Koh-i-noor, the great Indian diamond, now in the possession of her Majesty. It weighed about 130 carats, showed the marks of cutting on the flat and largest side, and appeared to correspond with the Koh-i-noor." Another diamond, which singularly corresponds with the Koh-i-noor, is the great Russian diamond: and it is not improbable that they all formed one crystal, and that, when united, they would, allowing for the detaching of several smaller pieces in the process of cleaving, make up the weight described by Tavernier.

Note.—Since the date of my lecture the recutting of the Koh-i-noor has been successfully accomplished by the Messrs. Garrard. The brilliancy and general appearance of the diamond have been much improved, but the weight has been reduced more than a third. As exhibited in the Crystal Palace, the Koh-i-noor weighed 186-1/16 carats. I am indebted to Messrs. Garrard for the following account:—

"In cutting diamonds from the rough, the process is so uncertain that the cutters think themselves fortunate in retaining one-half the original weight. The Koh-i-noor, on its arrival in England, was merely surface cut, no attempt having been made to produce the regular form of a brilliant, by which alone lustre is obtained. By reference to the figures,



Upper Surface. Under Surface.

THE KOH-I-NOOR IN ITS PRESENT STATE.

which are the exact size of the Koh-i-noor, it will be clearly understood that it was necessary to remove a large portion of the stone in order to

* "Les Six Voyages, &c., Seconde Partie, Paris, 1676." Tavernier, at page 249 of this volume, states the weight of the diamond to be 787½ carats; but at page 334 he calls it 793 carats.

obtain the desired effect, by which means the apparent surface was increased rather than diminished, and the flaws and yellow tinge were removed.

"The process of diamond-cutting is effected by an horizontal iron plate of about 10 inches diameter, called a *Schyf*, or *mill*, which revolves from two thousand to three thousand times per minute. The diamond is fixed in a ball of pewter at the end of an arm, resting upon the table in which the plate revolves; the other end, at which the ball containing the diamond is fixed, is pressed upon the wheel by moveable weights at the discretion of the workman. The weights applied vary from 2 to 30 lbs. according to the size of the facets intended to be cut. The recutting of the Koh-i-noor was commenced on July 16th, 1852, his Grace, the late Duke of Wellington being the first person to place it on the mill; the portion first worked upon was that at which the planes P and F meet, as it was necessary to reduce the stone at that part, and so to level the set of the stone before the table could be formed; the intention being to turn the stone rather on one side, and take the incision or flaw at E, and a fracture on the other side of the stone, not shown in the engraving, as the boundaries or sides of the girdle. The next important step was the attempt to remove an incision or flaw at C, described by Professor Tennant and the Rev. W. Mitchell as having been made for the purpose of holding the stone more firmly in its setting, but pronounced by the cutters (after having cut into and examined it,) to be a natural flaw of a yellow tinge, a defect often met with in small stones. The next step was cutting a facet on the top of the stone immediately above the last-mentioned flaw; here the difference in the hardness of the stone first manifested itself, for while cutting this facet, the lapidary, noticing that the work did not proceed so fast as hitherto, allowed the diamond to remain on the mill rather longer than usual, without taking it off to cool: the consequence was, that the diamond became so hot from the continual friction and greater weight applied, that it melted the pewter in which it was imbedded. Again, while cutting the same facet, the mill became so hot from the extreme hardness of the stone, that particles of iron mixed with diamond powder and oil became ignited. The probable cause of the diamond proving so hard at this part is, that the lapidary was obliged to cut directly upon the angle at which two cleavage planes meet, cutting across the grain of the stone. Another step that was thus considered to be important by the cutters was removing a flaw at G. This flaw was not thought by Professor Tennant and Mr. Mitchell to be dangerous, because, if it were allowed to run according to the cleavage, it would only take off a small piece, which it was necessary to remove in order to acquire the present shape. The cutters, however, had an idea that it might not take the desired direction, and, therefore, began to cut into it from both sides, and afterwards directly upon it, and thus they succeeded in getting rid of it. While cutting, the stone appeared to become harder and harder the farther it was cut into, especially just above the flaw at A, which part became so hard, that, after working the mill at the medium rate of 2400 times per minute, for six hours, little impression had been made; the speed was, therefore, increased to more than 3000, at which rate the work gradually proceeded. When

the back (or former top) of the stone was cut, it proved to be much softer, so that a facet was made in three hours, which would have occupied more than a day, if the hardness had been equal to that on the other side; nevertheless, the stone afterwards became gradually harder, especially underneath the flaw at A, which part was nearly as hard as that directly above it. The flaw at N, did not interfere at all with the cutting. An attempt was made to cut out the flaw at A; but it was found not desirable, on account of its length. The diamond was finished on September 7th, having taken thirty-eight days to cut, working twelve hours per day without cessation."

By permission of her Majesty, models of the Koh-i-noor, as it arrived in this country and in its present state, have been placed with the diamonds in the mineralogical department of the British Museum.

3. *Abstract of a Meteorological Register, kept at the Tennessee Institution for the Deaf and Dumb, Knoxville, Tennessee, for the year* 1852; by O. W. Morris, Principal. The Latitude is 35° 56′; Longitude west from Washington, 6° 57′; elevation above tide, 960 feet.*

Months.	BAROMETER.				THERMOMETER.				Cloud-iness.	Prev'g Winds
	Maximum.	Minimum.	Means.	Range	Maximum.	Minimum	Means.	Range		
					°	°		°		
Jan.	19, 29·692	5, 28·434	29·127	1·258	31, 63·2	19, −4·	30·513	67·2	4·44	N.W.
Feb.	19, 29·431	28, 28·474	29·073	·957	24, 67·	3, 21·6	42·810	45·4	5·49	W.
March.	3, 29·454	28, 28·434	29·027	1·020	26, 79·8	20, 21·	52·401	58·8	5·02	W.
April,	10, 29·146	5, 28·348	28·856	·798	30, 79·	1, 32	50·423	47·0	4·90	W.
May,	6, 29·442	27, 28·827	29·054	·615	2, 84·	21, 43·2	64·689	40·8	4·74	W.
June.	14, 29·391	8, 28·732	29·094	·659	23. 86·6	11. 50·5	68·498	36·1	4·32	W.
July.	21, 29·292	30, 28·833	29·068	·459	8&24, 92·5	3, 59·	74·096	33·5	4·01	N.
Aug.	30, 29·318	27, 28·775	29·086	·541	13, 87·5	7, 55·9	69·953	31·6	5·44	N.E.
Sept.	18, 29·316	11, 28·747	29·120	·569	2, 85·4	14, 45·	66·452	40·4	4·90	N.E.
Oct.	3, 29·360	9, 28·779	29·124	·581	8, 83·2	25, 39·6	59·656	43·6	3·79	N.E.
Nov.	29, 29·447	26, 28·48[illegible]	29·048	·951	5, 71·2	19, 24·6	44·119	46·6	5·10	N&NE
Dec.	29, 29·425	7, 28·667	29·078	·758	20, 64·8	14, 25·1	44·511	39·7	6·53	N.E.
An. M'us.	29·392		28·628	29·063	·764	78·68	43·37	55·667	45·511	4·89	W.

The mean temperature of the winter months of 1851–1852, was 39°·279; of the spring months, 58°·838; of the summer months, 70°·849; of the autumnal months, 56°·742. The highest degree of the ordinary thermometer, as noted, was twice 92·5° in July; the lowest was −4° in January. The winter of '51–'52 was an uncommonly cold one for that country.

Rain fell on 114 days; it was accompanied by thunder and lightning on 32 days, on some of them twice, and on one day three times, with very heavy thunder and vivid lightning. On the third of May the lightning struck an oak tree within one hundred feet of the building.

Snow fell on 11 days, to the depth of 3½ inches; the first snow in the winter was on the 12th of December, and the last snow in the Spring was on the 2nd of April.

The mean relative humidity of the atmosphere for the winter months was 72·96; for the spring months, 66·89; for the summer months, 78·52; for the autumnal months, 77·79; and for the year it was 73·99. The most prevalent wind was the west, the next the northeast, and the next the northwest.

* In the columns Maximum and Minimum, the day of the month precedes the statement of the observation.

The Aurora Borealis was observed on four nights only, viz: Jan. 19th, Feb. 19th, April 22d, and May 9th; that on the 19th of January was the only one that exhibited any perceptible motion, the others were a steady light; that on the 9th of May was a faint light, only a few degrees above the horizon. A solar halo was noticed once, and lunar haloes on fourteen nights. Both solar and lunar haloes were followed by rain or snow within a short period of time.

A shock of an earthquake was felt on the 27th of October, at 11.30 A. M., barometer 29·171, thermometer 71·8°. At 6 A. M. the barometer was 29·224, and at 10 P. M. 29·212;—the direction of the wave was from west to east; the air was very sultry, with no perceptible wind, the atmosphere hazy and smoky. The loose boards on the buildings rattled, and so did crockery, &c. on the shelves, looking glasses and other pendant articles swung from side to side, horses that were trotting or walking in the road involuntarily stopped, and the domestic animals seemed stupefied and in some instances frightened. The jar was accompanied by a noise resembling distant thunder for a second or two, then a loud report like firing heavy artillery, followed by a sound resembling the reverberations of cannon among the mountains, dying away in the distance. Many people throughout the country were much alarmed, and ran out of their houses.

At 8 o'clock P. M. of the same day there was a very bright meteor; its course was from N. W. to S. E., leaving a track of bright light, 8 or 10° in length, which continued several seconds; it was at some distance above the earth; no report was heard when it disappeared.

4. *On the Binocular Microscope, and on the Stereoscopic Pictures of Microscopic Objects;* by Professor C. Wheatstone, F.R.S., (Quart. Jour. Mic. Sci., No. iv, July, p. 99.)—In Section 11 of my first Memoir on Binocular Vision, published in the Philosophical Transactions for 1838, I have alluded to the illusions to which microscopic observers are liable, from their inability to judge correctly the relief of objects when one eye only is employed. This indetermination of the judgment exists whenever a shadowless object is regarded with a single eye. Frequently an elevation appears as a depression, a cameo as an intaglio, a hollow pyramid (as a crystal of muriate of soda) as a pyramid in relief, &c., and *vice versâ*; but this indecision is entirely removed when the object is viewed with both eyes simultaneously. No mistake, if the object be a near one, can then be made with regard to its relief; and the relative positions of every point, in depth as well as in length and breadth, can be directly determined.

The stereoscope affords a convincing proof that the two projections of an object presented to the two eyes, suggest the real object far more effectively to the mind than a single projection to one eye does; and those who have paid much attention to the appearance of binocular pictures in the stereoscope, will not have failed to remark, that not only is double vision of importance to enable us more accurately to judge of the relief of bodies, but it also occasions us to perceive things which pass entirely unnoticed when monocular pictures alone are regarded. Fully impressed with these views, and convinced, from the reasons above stated, that a binocular microscope would possess great advantages over the present monocular instrument, I, shortly after the publica-

tion of my first memoir, called the attention both of Mr. Ross and Mr. Powell to this subject, and strongly recommended them to make an instrument to realize the anticipated effect; their occupations, however, prevented either of these artists from taking the matter up. The year before last, previous to the publication of my second memoir, I again urged Mr. Ross, and subsequently Mr. Beck, to attempt its construction, and for a short time they interested themselves in the matter, but ultimately relinquished it for want of time, and in my opinion overestimating the difficulties of the undertaking.

It appears, however, from a communication in the 'American Journal of Science' of January 1853, which has been reprinted in the last number of the 'Microscopical Journal,' that such an instrument has been actually constructed by Professor J. L. Riddell, of New Orleans, and the results expected have been obtained. The method Mr. Riddell employs is similar to the one I recommended to Mr. Beck. After the rays from the object pass through the compound object-glass in the usual manner, he deflects them by means of a system of rectangular prisms into two directions parallel to the original, and sufficiently separated for the images to be seen by each eye. As in this arrangement there must be a considerable loss of light, I have proposed another which will not have this disadvantage, and which I will shortly submit to the Society.

A binocular microscope is, however, by no means a novelty, and its invention dates nearly two centuries back. I have found, in the library of the Royal Society, a work entitled 'La Vision parfaite, ou les Concours des deux Axes de la Vision, en un seul point de l'Objet: Par le P. Cherubin d'Orléans, Capucin.' This work was published at Paris in 1677, and in it eight chapters and a plate are devoted to a minute description of the instrument, which he informs us he constructed, and presented to the Dauphin. The following is an extract from the Preface:—

"Some years ago I resolved to effect what I had long before premeditated, to make a microscope to see the smallest objects with the two eyes conjointly; and this project has succeeded even beyond my expectation, with advantages above the single instrument so extraordinary, and so surprising, that every intelligent person to whom I have shown the effect has assured me that inquiring philosophers will be highly pleased with the communication. For this reason I have determined to make it the principal subject of the present work."

And the second part, which contains a description of the instrument, is thus headed:—

"Section the first, in which is taught the method of constructing the newly-invented microscope to see the smallest objects very agreeably and conveniently, represented entire to the two eyes conjointly, with a magnitude and distinctness which surpasses everything which has been hitherto seen in this kind of instrument."

In the Père d'Orléans binocular microscope, two object-glasses have their lateral portions cut away so as to allow of close juxtaposition, and these nearly semi-lenses are so arranged, that their axes correspond with the two optic axes passing through the tubes containing the eye-

pieces. The author's aim in its construction was solely the reinforcement of the impression by presenting an image to each eye, for he assumes, according to the then prevalent error, that vision by the two organs conjointly is naturally and necessarily unique, from the perfect conformity of all the homonymous parts of the two images of the object on the two retinæ. The real advantage of such an instrument entirely escaped his attention; viz., that of presenting to the two eyes the two *dissimilar* microscopic images of an object, under precisely the same circumstances as the two unlike images of any usual object is presented to them when no instrument is employed, by which simultaneous presentment the same accurate judgment as to its real solid form, and the relative distances of all its points, can be as readily determined in the former case as in the latter.

In the construction of a binocular microscope there is one thing especially to be attended to—viz., that the images be both direct, for in this case only a true stereoscopic representation will be obtained. If the images, on the contrary, be inverted, a pseudoscopic effect would be produced which will give a very erroneous idea of the real form. The reason of these effects is fully explained in Sections 5, 10, 22, 23, of my Memoirs. The reversal of the images by reflection from mirrors or reflecting prisms, will produce the same result as to the stereoscopic and pseudoscopic appearances as their inversion by lenses. The binocular microscope constructed by the Père d'Orléans was pseudoscopic, though he describes one which, had it been made, would have been stereoscopic; he was, however, quite unaware that there would be any difference of this kind between them. The pseudoscopic effects when inverted images are presented, and the natural appearances when erecting eye-pieces are employed, have not escaped the observation of Mr. Riddell.

Besides actual inspection by means of the binocular microscope, there is another way in which the advantages of binocular vision may be applied to microscopic objects. The beautiful specimens of photography, reproducing the highly magnified images of objects, inserted in a recent number of the Microscopic Journal, makes one regret that they were not accompanied by their stereoscopic complements. A very simple modification of the usual microscope would fit it for producing the two pictures at the proper angles; all that is necessary is to cause the tube of the microscope to move independently of the fixed stand round an axis, the imaginary prolongation of which should pass through the object. A motion of 15° would include every difference of relief which it would be desirable to have, and it is indifferent in what direction this motion is made in respect to the stand. The pair of stereoscopic pictures may be obtained by a still simpler method, which requires no alteration in the microscope; the object itself may be turned round on an imaginary axis within itself, from 7° to 15°. But this method is inapplicable unless the light be perfectly diffused and uniform so as to avoid all shadows, the presence of which would give rise to false stereoscopic appearances. In the former case, where the object remains stationary and the tube moves independently of the frame, the arrangement of the light so as to cast single shadows might be an advantage, and assist the visual judgment.

5. *On the periodic and non-periodic variations of Temperature at Toronto in Canada from* 1841 *to* 1852 *inclusive;* by Colonel EDWARD SABINE, R.A., Treasurer and Vice-President of the Royal Society, (Phil. Mag., [4], v, 376.)—The principal object of this communication is to make known the non-periodic variations of temperature for every day in the twelve years, from 1841 to 1852 inclusive, at Toronto in Canada. The non-periodic variations are those differences of the temperature from its mean or normal state which remain after all the known periodical variations are allowed for, and are such as are generally accompanied by peculiarities of wind or of other meteorological circumstances. Recent investigations have led to the inference that opposite conditions of weather prevail simultaneously in the same parallels of latitude under different meridians, and that in particular Europe and America usually present such an opposition, so that a severe winter here corresponds to a mild one there, and *vice versâ;* and recent theories of the distribution of heat on the surface of the globe profess to furnish the explanation. To place the facts on indisputable ground, it is requisite that a comparison should be made of unexceptionable records of the non-periodic variations in Europe and America, continued for a sufficient time to afford a proper basis for inductive generalisation. Toronto, from its latitude 43° 40′ N. and inland situation, is well suited to supply such a comparison with stations in the middle parts of Europe where similar records have been kept; and the twelve years embraced by the observations, viz.: 1841 to 1852, have been years of unusual meteorological activity in Europe.

Details are given in the commencement of the paper showing the care bestowed on the examination of the thermometer employed, with a standard divided "*à l'échelle arbitraire*," by the method of M. Regnault; as well as the precautions adopted for its fair exposure, and for its protection from rain and radiation. The observations were made by the non-commissioned officers of the detachment of the Royal Artillery employed in the duties of the observatory.

The period of twelve years comprises two series, in one of which the thermometer was observed hourly, and in the other less frequently, each observation in the second series receiving however a correction to the mean temperature of the day furnished for every hour and every day of the year by the first series. The two series, each of six years, are separately discussed; from the first series equations are derived from the mean monthly temperatures by the method suggested by Bessel (Astron. Nach. No. 136), whereby the most probable values of the temperature, on every day and every hour, are computed corresponding to the whole body of the observations. These the author regards as approximate normal values, and by comparing with them the actual daily temperatures,—which in the first six years are the means on each day of twenty-four equidistant observations, and in the second six years the means of all the observations made on each day, each observation having been corrected for the hour in the manner described,—the non-periodic variations for every day in the year are obtained and are given in a table.

From the approximate normal temperatures the author has represented in a plate the phenomena of the temperature at Toronto, according

to a method which, if applied to the different meteorological elements and in different localities, might, he thinks, materially facilitate their intercomparison. This method, in which three variables are represented, one being dependent on the other two, is essentially the same that has been long used in magnetic maps, and in the ordinary isothermal maps; from which latter however it differs in this respect, that, whereas in the ordinary isothermal maps the two variables on which the variation of temperature is dependent are the geographical latitude and longitude, in the present case the two variables are the hour of the day and the day of the year. The variation of temperature is here referred therefore to *time* and not to *space;* a distinction which the author proposes to convey by employing the term Chrono-Isothermals, as applicable to lines of this description. From the delineation in the plate, and from the tables contained in paper, many characteristic and some peculiar features of the climate and meteorology of the part of the North American continent in which Toronto is situated, are readily perceivable. Several instances are pointed out; amongst these may be noticed the peculiar anomaly of the North American winter, which is very conspicuous in the plate; and the absolute as well as relative *variability* of the temperature at different seasons of the year, exhibited by means of a numerical index analogous to the probable error of the arithmetical mean of a number of partial results, and deduced in a similar manner from the differences of individual years, months, and days, from their mean values: whence it appears, in respect to the annual temperature, for example, that in any particular year there is an equal probability that its mean temperature will fall within the limits of 43°·8 and 44°·6, as that it will exceed those limits on either side.

Finally, the author has shown the "Thermic Anomaly" (as it has been recently termed) of the monthly and annual temperatures at Toronto by comparison with the normal temperatures computed by Dove (Verbreitung der Wärme, 1852), for the parallel of 43° 40′ N. from 36 equidistant points on the parallel; from which comparison it appears that after allowance has been made for the elevation above the sea (342 feet), every month of the year is colder than the normal temperature of the same month in the same parallel; that the thermic anomaly reaches its extreme in February, when it exceeds 10° of Fahrenheit; and that on the average of the whole year it is little less than 6°.

6. *On Periodical Laws in the larger Magnetic Disturbances;* by Captain Younghusband, R.A., F.R.S., (Phil. Mag., [4], v, 379).—In this communication the author has arranged, in tables, the disturbances of the magnetic declination at St. Helena and the Cape of Good Hope, for the purpose of exhibiting the systematic laws by which those phenomena are regulated, which were long described as irregular variations, because they were of occasional and apparently uncertain occurrence.

The frequency of the disturbances, and their amount, whether viewed separately as easterly or westerly movements, or as general abnormal variations (easterly and westerly being taken together), is shown to be dependent upon the hour of the day, the period of the year, and upon the year of observation. This dependence upon the year of observation affords additional testimony of a periodical variation in the magni-

tude of magnetic changes of the same character as that which has been found to exist at other places, and which has been considered to be coincident with variations of the solar spots.

The disturbances of larger amount only are noticed; those observations which differed by 2·5 scale divisions (1′·8 in arc at St. Helena, and 1′·9 in arc at the Cape) and upwards, from the normal place, were separated from the others and the values of the differences taken; there were therefore two series of figures to be dealt with, viz: the number of disturbances, and the aggregate amount of disturbance. These were separated into disturbances of the north end of the magnet towards the east and towards the west, and the effect of each considered separately.

The periodical character of disturbances at St. Helena and the Cape in a cycle of years is indicated so far as the limited extent of the observations would permit; sufficient however to point to the year 1843 as that of least disturbance at these two places, by showing a regular decrease from the previous years, and an increase in every succeeding year of observation. Though the hourly observations were discontinued before 1848, the year which Colonel Sabine has shown to be that of periodical maximum, (as 1843 was that of minimum magnetic activity at Toronto and Hobarton,) the observations now discussed are shown to be quite consistent with this period, and thus tend to establish it as a general law of magnetic phenomena. In the aggregate of each year the disturbances towards the west are shown to preponderate over those towards the east, both at St. Helena and the Cape of Good Hope; a similar preponderance of westerly over easterly has been found in every year of observation at Hobarton, but at Toronto the easterly disturbances exceeded the westerly both in number and amount in every year.

Arranging the disturbances into the several *months* of their occurrence, the greatest disturbance is found to occur in January and the least in June at St. Helena and the Cape of Good Hope; the same months being those of greatest and least disturbance at Hobarton, whereas at Toronto, *both* January and June are months of minimum disturbance, the maxima disturbance occurring there in April and September.

From this identity of the epoch of greatest and least disturbance,—at St. Helena, where the months of January and June are not those of opposite seasons, viewed either with respect to the sun's extreme altitude or to extreme periods of temperature,—at the Cape, situated in S. latitude 33° 56′,—and at Hobarton in S. latitude 42° 52′,—and contrasting this identity with a different law at Toronto in N. latitude 43° 39′, the author infers that the principal causes which produce an annual period of disturbance are not dependent upon local seasons. It is likewise pointed out that about the period of the equinoxes there is a tendency to maximum disturbances at all the stations, producing absolute maxima at Toronto, faintly but systematically indicated at the other stations.

The westerly disturbances were found to exceed the easterly in every month in the year at St. Helena and the Cape, which agrees with the results deduced from the Hobarton observations, while it appears

from the observations at Toronto that the easterly disturbances exceeded the westerly in every month. The average value of a westerly disturbance is greater than that of an easterly in every month at St. Helena and the Cape of Good Hope. The disturbances at Hobarton again coincide with this result; and in a slight and less perfectly marked degree, Toronto has the same peculiarity.

Arranging the disturbances into the several *hours* of their occurrence, the hours of the *day* are found to be those of greatest disturbance in a very considerable degree; the sum of the ratios, during the twelve hours of the day, being about seven times as great as the sum of those in the twelve hours of the night at St. Helena, and about 2·6 times as great at the Cape of Good Hope; while at Hobarton the sum of the twelve night ratios slightly exceeded the day; at Toronto the excess was larger, viz: as 1·3 to 1. The laws of easterly and westerly disturbances, in relation to the local hours, are then examined separately. At St. Helena and the Cape, the easterly day-disturbances exceed the easterly night-disturbances, and the westerly day-disturbances exceed the westerly night-disturbances. These results are compared with those at Toronto and Hobarton.

At St. Helena, although but comparatively few disturbances occur during the night hours, those disturbances are almost all westerly (183 disturbances, in all, occurred in nine night hours during five years, of which 174 were westerly and but nine easterly). In the day hours the westerly only *slightly* exceed the easterly disturbances. At the Cape, the westerly excess is less in the night and greater in the day than at St. Helena, and the night excess much greater than the day excess.

At St. Helena, the fact of the disturbances being more frequent in the day than in the night is consistent in every month of the year; this appears worthy of remark when it is remembered that at St. Helena the curve of the diurnal variation of the declination is precisely reversed at two opposite periods of the year; in one case corresponding to the curve of diurnal variation in middle northern latitudes, and in the other to that in middle southern latitudes.

The mean effect of the disturbances which have been separated as described, and which comprise all of largest magnitude, is a *constant westerly* effect at every hour both at St. Helena and the Cape of Good Hope, acting more energetically in the night than in the day. At Toronto, the mean effect is westerly in the day and easterly in the night; at Hobarton, easterly in the day and westerly in the night.

7 *Mode of Determining the Optical Power of a Microscope;* by Prof. Harting, (Quart. J. Mic. Sci., July, 1853, 292.)—I conclude by noticing another method of testing the optical power of the instrument, which, although rather troublesome, appears to me among the best, permitting us, as it does, to ascertain with a great degree of accuracy and certainty, the utmost limits of penetrating and separating power possessed by a microscope, and hence easily to express numerically its optical qualities in the most varied circumstances.

This method consists simply in subjecting to observation under the microscope the dioptric images of certain minute objects instead of the objects themselves. These images can be diminished at pleasure by withdrawing to a distance from the lens the object which forms them;

and hence we have it in our power to measure the extreme limits at which the object continues to be visible.

For the formation of the dioptric images achromatic object-glasses might be used; but even where those of the shortest focal length are employed, the object whose image it is required to form must be placed at a great distance. This would cause various difficulties, and only be practicable with a microscope placed horizontally—unless, indeed, the object selected were very minute, in which case the accurate determination of its diameter (from which that of its image must be afterwards deduced) would be rendered difficult.

Small air-bells in a fluid are for this purpose far better. I employ by preference a watery solution of powdered gum arabic, which always contains numbers of such air-bells originating in the air entangled among the particles of the powder. The water employed should have stood for a considerable time freely exposed to the air, or been shaken up with the air for some time; for when we use water which is not saturated with air, the bubbles in the fluid gradually become smaller, and images formed in them decreasing in magnitude, cause errors in the subsequent measurements, as we shall actually find to be the case.

A drop of the fluid must then be placed on a clean glass object-slide, and covered with a good clear mica plate, a ring shaped piece being interposed, in order to prevent the flattening of the air-bells by pressure. The object-slide is then placed under the object-glass upon the stage of the microscope, and an air-bell of suitable size for the formation of the images is sought for. All do not give images of the same degree of sharpness; a peculiarity dependent on the fact that some air-bells are in contact with the covering-plate, and consequently have their spherical form disturbed to some extent, or on the presence of small molecules in the fluid above or beneath the air-bell, or even in its interior, causing some haziness of the image, just as defective polish of a glass lens would do. It will, however, be always easy to find some which will form images of the utmost distinctness and purity.* This may be ascertained in the first instance by holding between the mirror and stage some easily recognized object, *e. g.* a piece of paper or the like. The image is always formed on the under surface of the air-bell, which must consequently be brought nearer to the object-glass than when it is desired to bring its margins into focus.

The object whose image is to be the subject of examination should be placed upon an apparatus, which can be moved upward and downward in the space between the mirror and stage. In some microscopes this can hardly be done, either from the space being too limited, or in consequence of the drum-like form of the foot of the microscope which quite envelops the space. If such microscopes, in place of a mirror, be provided with a reflecting prism, the object may be placed opposite the side external to the microscope. The instruments best adapted for

* The following example will demonstrate this. I brought a printed page of a book to such a distance from an air-bell that the length of the image of the whole page was 1-7th millimetre = about 1-180th of an inch, and that of the image of each letter about 1-480th millim. = 1-12,000th of an inch. In spite of their minuteness, these images, formed by reflected light, possessed such clearness and sharpness, that under a magnifying power of 154 diameters the whole page was without difficulty legible.

the manipulation which we are describing are, however, those whose illuminating apparatus consists of a mirror and converging lens, which can be shifted up or down. The lens being removed from the ring which supports it, the object is substituted in its place. The relative magnitudes of object and air-bell must be such that the image shall be exceedingly minute when the object is tolerably near to the stage. On afterwards increasing the distance between the object and air-bell, it is not difficult to find the limit at which the image (under a given magnifying power) is barely visible.

Of course it is impossible to measure *directly* the dimensions of this most minute visible image, for our best micrometric methods will here be found of no avail. Yet their size may be estimated with extreme accuracy in the following manner. At the same distance from the air-bell and in place of the object used, substitute another body, such as a piece of card, of 4 to 5 centimetres = $1\frac{3}{5}$ths to 2 inches diameter, which has been exactly measured. Let this be now again measured (by some of the micrometric methods elsewhere alluded to*), just as if it were a real object. By dividing the real diameter by the apparent diameter, the amount of diminution is found; and this is the same for all objects at a like distance from the air-bell. We have, consequently, nothing to do, in order to find the amount of diminution of the image of the more minute object, but to divide its *true* diameter by the figure expressing the diminishing power.

For example, let the true diameter of the greater object be 5 centimetres = to 1·969 English inches, and the diameter of its image = 32·2 micromillimetres,† = ·00127 English inches, then the figure expressing the amount of diminution will be $\frac{1\cdot969}{\cdot00127} = 1553$ very nearly. If now the smaller object have a diameter of 175 micromillimetres = ·00689 English inches, then must its image at the limit of vision be in diameter = $\frac{\cdot00689}{1553} = \cdot0000044$, or about $\frac{1}{225,000}$th of an English inch. When exact micrometric methods are employed, it is easy in this way to estimate the diameter of an image even to millionth parts of a millimetre, *i. e.* to 25,400,000th parts of an inch.

As for the object suitable for these investigations, it is plain that we have an extensive choice. To find the limit of vision for bodies of a round or long thread-like form, grains of pearl sago, or vegetable bodies, such as mustard-seed or the pollen-granules of many plants, hairs of animals, metallic wires, &c., may be employed. Small round openings and chinks may serve for the determination of the visibility of positive images of light. In the last case, care must of course be taken, by means of suitable screens, to shut off all light except what passes through the aperture. To determine the defining power, metallic wire gause is a suitable object, or two holes placed near each other in a black metallic plate. The images of such objects resemble exactly a double star viewed through a telescope (kijker). The bodies may likewise be placed in different circumstances in order to ascertain the influence of these upon the limits of vision. Thus we may use as an object a very

* See translation from *Het Mikroskoop* in Monthly Journal of Medical Science, June, 1852. p. 453, *et seq.*

† The micromillimetre is equal 1-1000th millimetres = ·0000394 English inches. See *Monthly Journal*, June, 1852, p. 456.

thin glass capillary-tube placed in water, and compare it with tender organic tubes and vessels, which may also be seen in water, but whose limit of visibility is of course far more circumscribed than that of absolutely opaque objects.

In fact this method admits of innumerable variations, and is consequently of most extensive application. Besides, when proper precautions are taken, it gives results perfectly sure and comparable. Especial care is however requisite in the mode of illumination. For it is certain, that when the field has a clear white ground, the contrast causes minute opaque bodies (*i. e.* objects which are dark by transmitted light) to continue visible, which against a grayish or light-blue background could not be seen. Hence it is by no means indifferent to receive on the mirror light from a white cloud, from a dull overcast, or clear blue sky. Artificial light cannot be used in these experiments, for the image of the flame becomes diminished like the object, and hence a clear field of view is not to be obtained. The observations must consequently be made by daylight; and whenever comparable results are sought for, the mirror should always be directed to the clear, blue, cloudless sky—this being a distinct atmospheric condition to which others in similar circumstances may refer in conducting the same experiment. The mode of ascertaining the limit of vision, with a given amount of illumination, may be gathered from different examples in the body of this work. It will likewise be found that for all such observations, even when the highest magnifying powers are employed, the *flat* mirror is perfectly sufficient, since in the image in the field of view formed by the air-bell, all the rays proceeding from the mirror are united and constitute an object of considerable luminous intensity.

8. *On an Instrument for taking Soundings;* by F. Maxwell Lyte, Esq., (Phil. Mag., [4] vi, 344.)—I see, from what Dr. Scoresby has brought before the Association at Hull, that there seems to be some difficulty about obtaining correct soundings in places where the currents are strong and flow in different directions at the different points of depth, causing the line to assume different curves in its descent; and when it comes to be measured over, after the weight has reached the bottom and been hauled up again, the measurement gives no approximate idea of the real depth. Now it is plain that this mensuration of the depth of water might be as well made by estimating its vertical pressure, as, in measuring the height of mountains, we measure the barometrical pressure of the air; and so I would propose to do it by an instrument constructed as follows :—

An accurately constructed tube of gun-metal or brass, or some metal not very easily corrodible by salt water, has a glass tube fitted on to it on the top by a screw joint, and again on the top of the glass tube is fitted a strong hollow copper ball by a similar screw joint. The lower tube, which we will call *a*, has a well-turned piston fitted to it, from which runs a rod which is only a trifle longer than the tube *a*, and just enters the tube *b* when the piston is at its lowest point. A well-made spring is placed in the tube *a* above the piston, and the tube *a* being narrowed at the top, so as just to admit the free passage of the rod, and the rod having a little button at its top, the piston is kept at its lowest point by the spring, except when sufficient pressure is applied from below

to compress the spring. The glass tube has a small ring fixed in it, just so as to stick at any point to which it is pushed, and the button at the top of the rod serves to push the ring straight, and the ring thus forms an index of the degree to which the spring has been compressed. The ball on the top serves as a mere reservoir of air to equalize the action of the apparatus as much as possible. The whole of this apparatus is enclosed in a wire cage for the sake of protection from blows. To graduate this apparatus, I let it down in a known depth of water, say ten fathoms, and having observed the point to which the ring in the glass tube is pushed, and having marked this point off, the ball is to be unscrewed, and with a small ramrod the ring is to be pushed down till it rests on the top of the piston-rod. The ball being replaced, the apparatus is sunk in twenty fathoms; after a similar manner it is sunk in thirty, and next in forty fathoms. This will test the accuracy of the apparatus; and the marks made on the glass tube *b* after each trial will give a scale from which the whole tube may be graduated, even to thousands of fathoms, if the tube be long enough or the spring strong enough. I have been induced to make this communication on account of the great use which may be made of such an apparatus.

9. *Louis Sæmann.*—Mr. Sæmann, who formerly travelled through the U. States, has arrangements at Paris for the sale of specimens or collections in Mineralogy, Geology, and Palæontology. To a thorough knowledge of minerals, Mr. S. unites a most excellent and obliging disposition; and his establishment is one of the largest in Europe. His address is Louis Sæmann, Comptoir Mineralogique et Palæontologique, Rue St. Andrè-des-Arts, No. 45, Paris.

10. *Cabinet of Minerals for sale.*—A large and excellent cabinet of minerals, and mineralogical works accompanying, is for sale at Washington. Address Fr. Markoe, Esq., Washington City. It is one of the best private collections in the country.

11. Obituary.—James E. Teschemacher died suddenly near Boston on the 9th of last November. Mr. Teschemacher, although engrossed with other cares, has been an unceasing and successful laborer in Science. He was an exact observer, and delighted in searching out with his microscope what passed unnoticed by others. He has contributed much to our knowledge of American minerals, both through the detection of rare forms of species and by his publications. A letter from him written but two days before his death, states his plans for research through the winter, and the progress he had already made in his chemical examination of the Chesterfield pyrochlore. He was most generous in communicating the results of his researches and allowing the free use of his Cabinet of Minerals to those who appreciated its excellencies. They were mostly miniature specimens, but often of rare interest. He has been laboring of late on the subject of the Fossils connected with coal, and the structure of the coal itself, and had collected much that was novel, which he was preparing for publication.

12. *Die Kreidebildungen von Texas, und irhe organischen Einschlüsse;* Von Dr. Ferd. Roemer. 4to. Bonn, 1852.—This work has already been briefly noticed in the September number of this Journal. The Introduction contains, 1. A sketch of the geographical situation and

general orographic condition of Texas; 2. General geognostic constitution of the country; 3. Diluvial and alluvial formations; 4. Tertiary formations; 5. Older or Palæozoic strata; and 6. Plutonic rocks.

The principal part of the volume is devoted to a geological description of the chalk formation of Texas, with an enumeration and description of its organic remains; which occupy eighty-eight pages of the volume. These are followed by descriptions of Palæozoic fossils, and of three species of fossil wood from the tertiary.

The cretaceous fossils figured occupy ten quarto plates, and number one hundred and twenty-four. Of these one hundred and one are new or yet undetermined species, and twenty-three are identical with species previously known. These are beautifully illustrated. The Palæozoic fossils number ten species, of which eight are new. These are chiefly of the carboniferous period, and we recognize them as forms which prevail farther to the north and west in the same formation. The three species of fossil wood from the tertiary, described by Unger are *Sillimania Texana*, *Roemeria Americana*, and *Thuyoxylon Americanum*.

The fossils here described and figured, had been already indicated in Dr. Roemer's previous work on Texas.*

The present work offers a very valuable accession to our knowledge of the American Cretaceous formation; contributing more species of fossils than have been published since the appearance of Dr. Morton's Synopsis in 1830. Dr. Roemer has given some valuable observations on the climatic influences upon the fauna of the chalk period, and has instituted comparisons between this formation in Texas and other parts of America, as well as with the same formation in Europe.

On comparing these figures of Texan cretaceous species with collections from Nebraska, a few degrees farther north, we are struck with their almost total dissimilarity. In the little which we already know of it, we have but a foreshadowing of what is yet in store for us when this formation, which extends from the Tropic of Cancer to the 48th degree of latitude, shall have been completely explored. H.

13. *Geological Map of Keweenaw Point, Lake Superior, Michigan;* by J. D. Whitney, assisted by S. W. Hill and W. H. Stevens.—This is a large pocket map of the Lake Superior Mining Region, 2 feet by 4 in its dimensions. It gives an admirable view of the Geological structure of the region, and is excellent in illustration of an article in this volume from the Report of Messrs. Whitney and Foster. The different rocks are indicated as usual by colors, and many facts of great geological interest are indicated by their arrangement, and the positions of the various metallic veins which intersect them. The map contains also a section across Keweenaw Point by Copper Falls and Northwestern Mines. It is invaluable to geological science as well as to the topographer and traveller.

14. *People's Journal;* Vol. I, Nos. 1 and 2, November and December, 1853. 32 pp. large 8vo.—This new popular monthly opens with an article on Willison's Hand Thrashing Machine. The Journal is de-

* Texas: mit besonderer Rucksicht auf deutsche Auswanderung und die physicalischen Verhältnisse des Landes nach eigener Beobachtung geschildert; von Ferdinand Roemer. Mit einen naturwissenschaftlichen Anhange und einer topographische-geognostischen Karte von Texas. Bonn, bei A. Marcus, 1849.

voted to Farming, Agriculture, Horticulture, Mechanics, and practical science, and is very fully and handsomely illustrated with cuts representing machinery, implements of husbandry, fruit, cattle, plans of buildings, the National Exhibition of Horses at Springfield, American steamships, etc. No. 2 contains 72 wood-cuts, some of them illustrating important inventions at the Crystal Palace. Price 50 cts. each 6 months.

E. Hitchcock, D.D.: Outline of the Geology of the Globe, and of the United States in particular; with two Geological Maps and sketches of characteristic American Fossils. 8vo. pp. 138. Plates. *Boston*, 1853. Phillips, Sampson & Co.

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